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CULTURAL EVOLUTION, SOUTHERN ATHAPASKANS, AND CHRONOLOGY IN THEORY

MORRIS E. OPLER

IN A RECENT ARTICLE Dr Betty J. Meggers discusses a "law of energy and cultural evolution" which she contends was first set forth in 1943.[1] "This law," she tells us, "is based on the recognition that all cultures are composed of three general classes of phenomena: technology, social organization, and philosophy. Of these, technology is primary and determines the content and form of the other two components. . . . The content of any particular culture depends on the amount of energy that can be controlled and the efficiency of the technological means of putting this energy to use." She asserts, however, that this "law" and the evolutionary theory of which it is the foundation have been used until now only "on a high plane of philosophical discussion" and have not been part of the "practical tool kit of the field anthropologist." The purpose of her paper, as she explains it, is "to dissent from the view that evolutionary theory has only limited and generalized applicability and to suggest how it can be used as a guide in understanding the dynamics of individual cultures."

In developing her point Dr Meggers offers a number of "tests" and examples of the kind of proof she has in mind. One of these "tests" or "cases" has to do with certain tribes of the Southern Athapaskans, where she sees a series of evolutionary developments or shifts in social organization growing out of differentials in the subsistence or energy realm. "Translated into the terms of the law of energy and the evolution of culture," she claims, "this correlation throws into relief a small segment of the process by which a minor alteration in the energy resource produces a minor change in social organization, . . ." Such differentiation, she thinks, "viewed in the larger framework of cultural evolutionary theory

1 Meggers, 1960.

. . . takes on broader significance and deserves the attention of other than specialists in kinship analysis. . . ."

The social organizational changes which Dr Meggers deems are of an evolutionary order and which she sees resulting from a difference in subsistence techniques are described mainly for the Chiricahua and Mescalero Apache on the one hand, and for the Jicarilla Apache, Lipan Apache, and Kiowa Apache on the other. The network of relationships and the sequence of events, as she describes and analyzes them, are essentially as follows:

The component Southern Athapaskan tribes developed recently from a common ancestry. Differences in the details of social organization must therefore be the result of evolution from a single form. Analysis suggests that certain of their present differences can be explained functionally as recent adjustments to habitats with varying subsistence resources. The Chiricahua and Mescalero, who were nonfarming, hunting and gathering peoples in a severe ecological region, had no coöperating units larger than local groups comprised of close relatives. Nuclear family relationships and extensions of nuclear family relationships were central to the social life of these tribes, and they placed greatest emphasis upon sibling solidarity. The Jicarilla, Lipan, and Kiowa Apache, the groups farther to the east, relied more on the buffalo hunt and/or agriculture. As a result of this augmented food supply (i.e., increased source of energy) these tribes developed a social organization in which generational opposition formed the basis for coöperation and training. In comparison with the Chiricahua and Mescalero the social organization of these tribes encouraged interaction between larger groups of relatives and between nonrelatives and placed greater emphasis on the band.

On the basis of some experience among Apache tribes, I should like to examine, for purposes of accuracy and clarification, the main points which Dr Meggers has sought to make. Perhaps a series of questions stimulated by Dr Meggers' presentation and the answers which seem indicated by the evidence will point to the issues involved.

1. Is it true, as Dr Meggers asserts, that differences in Apache social organization can, in all these cases, be correlated with differences in subsistence economy? The answer, as I read the evidence, is no. The Mescalero, and specially the eastern band of the Mescalero, were buffalo hunters.[2] In my judgment, the eastern band of the Mescalero did as much buffalo hunting as the western band of the Jicarilla. But the social organization of the eastern Mescalero does not differ from that of the western band of the tribe, nor does it differ from that of the non-buffalo-hunting Chiricahua in any important respect. It does vary somewhat, especially in

2 Castetter and Opler, 1936, pp. 8-9.

respect to the kinship system, from that of the Jicarilla, despite the close comparison that can be made between the subsistence techniques of the eastern Mescalero and western Jicarilla.

2. Is it true, as Dr Meggers implies, that the local groups of the Chiricahua and Mescalero were composed solely of relatives but that the local groups of the Apache tribes to the east (Jicarilla, Lipan, Kiowa Apache) were of more heterogeneous composition and thus permitted the inclusion of nonrelatives in the coöperating group? The answer is no. In every Apache group I have studied in the field, although the nucleus of the local group is the close relatives of the leader, nonrelatives, attracted by the advantages of the site or the quality of the leadership, are also present. This is just as true for the Chiricahua and the Mescalero as it is for the Jicarilla and Lipan.[3]

3. Is it true, as Dr Meggers would have it, that for coöperation and training the Chiricahua and Mescalero depend solely on the nuclear family and its extensions and lean heavily on sibling solidarity, while the Kiowa Apache, Lipan, and Jicarilla emphasize generational opposition and therefore encourage interaction between larger groups of relatives? This is not supported by the evidence either. For example, there is no Apache group more family-centered, no group where specific relatives have more clearly defined roles to play than the Jicarilla.[4] Many of these Jicarilla usages are teasing or rivalry relationships, but they apply to specific close relatives and cut across generation lines. Thus among the Jicarilla the mother's brother goads and challenges a boy during his formative years, but so does the boy's cross-cousin of the same sex. A grandfather is in a position of authority in respect to his grandchild; but an elder brother, too, commands the obedience of his younger brother, and a brother has a good deal of control over his sister. One cannot squeeze a generational principle out of these data. But there is another kind of regularity that runs through them. This is a principle of responsibility for the protection and instruction of close kin. This rule of family responsibility and the formal roles in respect to kin which implement it form a conspicuous feature of Jicarilla culture. At the ceremony for the newborn, the infant is handed to the father while the ceremonialist intones the words, "This is your child." A woman should not marry if her brother does not approve of the choice. Children may not play carelessly with riding gear while their father is raiding for horses, for this will affect his luck. A Jicarilla wife may not lay firewood in a crisscross fashion while her husband is away on an expedition; otherwise he will lose his sense of direction. The Chiricahua and Mescalero are not nearly

3 Castetter and Opler, 1936, p. 8; Opler, 1941, p. 25; Opler, 1946, p. 2.
4 Opler, 1946, pp. 7, 16, 35, 42-48, 61-67, 89-90.

so specific, restrictive, and formal about the roles and duties of particular kin. The Jicarilla feel that the maternal grandfather is the proper person to acquaint a grandchild with the sacred legends and traditional stories. The Chiricahua and Mescalero are content to bring together the children of the neighborhood and to let the best available raconteur instruct and entertain them. Specific kin figure prominently in the hardening and training of the Jicarilla boy; e.g., a boy should properly receive instruction in hunting practices from his maternal grandfather. The Chiricahua and Mescalero are more prone to call boys of the same age together and to place them under the supervision of seasoned and qualified warriors, regardless of relationship considerations.[5] All this seems considerably at variance with what Dr Meggers has led us to expect.

4. Is it true, as Dr Meggers would have us believe, that the Jicarilla, Lipan, and Kiowa Apache (since they supposedly relied less on the nuclear family and its extensions) emphasized and developed the band to a greater degree than did the Mescalero and Chiricahua? Again the answer is in the negative. The Kiowa Apache, for one thing, have had no bands of which we know.[6] We have fairly conclusive evidence that the bands of the Lipan were formed under duress, that the tribe divided at a time of military pressures.[7] There were definitely less separatism and band rivalry among the Jicarilla than among the Mescalero and Chiricahua. In fact, under the stimulus of Pueblo example, the Jicarilla bands tended to take on moiety attributes and to coöperate in ritual enterprises, such as the ceremonial relay race, with reduced indications of autonomy and rivalry. If I were asked to choose the Apache band which had the strongest sense of its separate identity from among the Apache groups I have studied, I would not name a band of the Jicarilla or a band of the Lipan but I would most certainly nominate the Eastern Chiricahua band. Their resistance to removal from their hereditary homeland continued until their leader, Victorio, and many of his followers fell in battle. Even in the 1930s, when I was among them, they were seeking legal means of recovering some of their former territories and re-establishing themselves.

5. Is it true, as Dr Meggers' theory requires, that the agriculture and buffalo hunting carried on by the Kiowa Apache, Lipan, and Jicarilla augmented the "energy resource" and therefore produced changes in the social system? I can find very little evidence that the kind of agriculture practiced by the Jicarilla and Lipan and the conditions under which the Kiowa Apache hunted the buffalo led to the more abundant life. It is interesting that the two Southern Athapaskan peoples

5 Opler, 1941, pp. 72-73.

6 McAllister, 1955.

7 Opler, unpublished field notes; Sjoberg, 1953, p. 7. Sjoberg, following early Spanish records, calls local groups "bands" and calls bands "divisions."

who were unable to stand alone, even in aboriginal times, were the Jicarilla and the Kiowa Apache. The Jicarilla had to ally themselves with the Southern Ute to stave off common enemies, and the Kiowa Apache had to attach themselves to the Kiowa in order to preserve themselves. Moreover, there is no convincing evidence that these more easterly Apache tribes were ever stronger in numbers than the Chiricahua and Mescalero. One would expect, if Dr Meggers' thesis is correct, that economic developments important enough to transform the kinship system and social organization would also have some recognizable effect on numbers and tribal viability! Perhaps the explanation lies in historical and psychological contexts that the formula-minded are not likely to discern. The Apache are recent comers to the Southwest and the southern Plains. By the time they had differentiated and established themselves, the aboriginal life of the entire area was under pressure and attack. So far as we know, the Lipan practiced no agriculture until they were missionized in the middle of the eighteenth century. The collapse of the missions, where they had found refuge, followed soon after this. From this point on, they were so constantly under military attack by both Indian and Caucasian foes that settled, agricultural life would have been impossible for them in any case. Moreover, their method of farming involved so few families and was so desultory and half-hearted that its impact on the economy must have been minimal.[8]

Nor are farming and the quest for the horses and buffalo of the Plains automatically to be considered an economic boon to the Jicarilla, either. The Jicarilla suffered from the attacks of Navaho raiders who were on their way to the east for horses or buffalo meat or who were returning from such expeditions. Since they lived at the edge of the Plains, they were vulnerable to the raids of Plains Indians as well. As tribes to the east were dislocated by the European and American advance, these attacks increased in number and violence. The buffalo hunt was an abundant source of food in theory, but in practice it often ended in an encounter with enemies in which more was lost than gained. Agriculture was of questionable economic importance prior to American occupation. Not all families engaged in it. Those who did plant did not do so regularly.[9] The methods were crude and the care given the crops was minimal. The best farm sites were near the Plains, and those who tended them were exposed to enemy attack. I have many accounts of fields abandoned because of danger and of camps and fields attacked because they were not abandoned in time. Perhaps the outstanding impression one gets about Jicarilla agriculture is the lack of complete commitment to it. It took relatively little coaxing to lure Jicarilla farmers away on a hunting or raiding

8 Sjoberg, 1953, pp. 82-83.

9 Opler, 1936, p. 202, pp. 206-207.

expedition. The purposes for which the main crop, maize, was raised must also be considered. There is no doubt that a good deal of it went to make *tiswin*, the corn beer over which the Apache are so enthusiastic. *Tiswin* leavened the social rather than the economic life. It is true that the Apache regard it as a food of sorts and have been known to feed it to babies because of their faith in its nutritive value, but from a purely objective point of view I doubt that even Dr Meggers would credit it with stimulating an upward evolutionary spiral.

I have mentioned the matter of commitment. In the case of tribes like the Jicarilla, who are marginal to two culture areas both geographically and in respect to subsistence techniques, we will do well to take a feature such as this into consideration. The Jicarilla never became fully adapted to the Plains and never obtained the greatest possible return from its resources.[10] But they were sufficiently Plains-oriented so that they did not enter into intensive farming, either. In my opinion this unresolved competition between two irreconcilable ways of making a living was a paralyzing force and left them little better off in material wealth and numbers than the Chiricahua, who applied themselves to their hunting and gathering with enthusiasm and without reservation. To assume—without attention to the geographical setting, amount of exposure to enemies, degree of commitment and participation, contacts with neighboring peoples, historic forces influencing the area, and much else—that agriculture or any other subsistence technique will automatically account for social organizational features is, to my mind, anthropology by incantation. I do not think this is the kind of anthropology in which we should engage, but I do think we shall have to cope with it and take note of it, along with the couvade, soul-snatching, stone-boiling, and infibulation.

6. Is it established, as Dr Meggers confidently states, that all the Southern Athapaskan tribes "have diverged from a common ancestry," that "their differentiation is relatively recent," and that "differences in details of kinship terminology and behavior must therefore be the result of evolution from a single form"? This is not now as certain as Dr Meggers' language would imply. Not long ago, Dr Harry Hoijer carried out an ethno-linguistic analysis of Athapaskan kinship terms and systems. The Southern Athapaskans, he concluded, "came in two waves . . . Navaho, San Carlos [Western Apache], Chiricahua, and Mescalero were the result of the first migration, while Jicarilla, Lipan, and Kiowa Apache came somewhat later and perhaps by a different route. This conclusion must of course be regarded as highly tentative; the evidence on which it is based is admittedly both slender and incomplete."[11] If Dr Hoijer's conclusions are correct, the contrasts

10 Opler, 1946, p. 3.
11 Hoijer, 1956, p. 324.

in social organization which Dr Meggers interprets in terms of differential evolution from a common form based on variations in subsistence techniques are better explained as the reflection of separate histories and separate experiences.

7. Whether or not Dr Hoijer is correct in his conclusions, is it reasonable to regard the differences between the Chiricahua and Mescalero on the one hand and the Jicarilla, Lipan, and Kiowa Apache on the other, as *evolutionary* in nature? In biology, where evolution is an established and respected doctrine (and, indeed, perhaps because of this) it is not assumed that every series of variations has evolutionary significance. I believe that the variations among Apache groups are of the greatest interest and that every attempt should be made to record and explain them. But I would like to know what new or more complex social forms developed among the groups we have named which permit us to speak so assuredly of an *evolution* of social organization. All of these tribes remained small in size and clanless. The fundamental kinship unit for all of them was the extended domestic family with matrilocal residence. Affinal ties were strong, and the levirate and sororate guaranteed the maintenance of the family against disruption. Whenever terrain or conditions permitted, local groups were established at favorite sites under the leadership of some respected family head. Central authority was weak. Indeed, the fortunes of local group leaders rose and fell rapidly. Families protected and even disciplined their own members. Feuds between families were common. Occasionally a local group leader was so outstanding that he was recognized as a band leader, but more often a number of local group leaders led the band when a large gathering took place. Tribal solidarity and tribal consciousness were poorly developed. This is a descriptive formula that pretty well fits any of these five tribes. It is regrettable that the term "evolution" ever became a dirty word to some anthropologists. Yet it does not repair matters to reduce it to a meaningless word by loosely applying it to every ripple of the ethnographic pool. One wonders whether it is possible these days to warm one's hands over a fire without precipitating a learned treatise on the evolution of calefaction, complete with impressive references to E, T, and C.

Because I do not believe that the amount and kind of agriculture in which the Jicarilla engaged revolutionized their economy or transformed their social organization, this does not mean that I consider the Jicarilla agricultural complex unimportant. On the contrary, its presence and interrelations with other aspects of the culture were of great moment. But, though its material influence should not be ignored, its main impress was left, not on the economy or the social organization, but on religion and symbolism. And this I explain by the proximity and example of the Pueblo Indians, with their rich agriculturally oriented ceremonialism.

From an examination of the accuracy of Dr Meggers' statements I should like to turn to her claims of originality and to the dates she gives for the appearance of certain concepts and doctrines. Dr Meggers, it will be remembered, asserts that "the law of energy and cultural evolution" was first "set forth" in 1943. This law, she explains, ". . . is based on the recognition that all cultures are composed of three general classes of phenomena: technology, social organization, and philosophy. Of these, technology is primary and determines the content and form of the other two components. . . . The content of any particular culture depends on the amount of energy that can be controlled and the efficiency of the technological means of putting this energy to use. Expressed in a simple formula, E(nergy) $\times$ T(echnology) $\rightarrow$ C(ulture). The implications of this formula are threefold: (1) if there is no increase in energy (E) or improvement in technology (T), the culture (C) will remain stable; (2) if either energy or technology or both are increased or improved, the culture will increase in complexity; (3) if either the energy or the technology or both are diminished, the culture will decline in complexity."[12]

Can Dr Meggers be serious in telling us that this point of view was first "set forth" in 1943 and that she is embarking on a fresh scientific venture in applying it to specific cultures? In the interests of time perspective I should like to quote some passages from the second (1925) English edition of a work first published in Russian in 1921.

> If the relation between society and nature remains the same; i.e., if society extracts from nature, by the process of production, precisely as much energy as it consumes, . . . the society will mark time, and there results a state of stable equilibrium. . . . If we consider human society . . . and assume that the relation between it and nature is altered in such manner that society—by means of production—extracts more energy from nature than is consumed by society (either the soil becomes more fruitful, or new tools are devised, or both) this society will *grow* and not merely mark time. . . . Or in the case of society, let us assume that the relation between it and nature has been altered in such manner that society is obliged to consume more and more and obtain less and less (the soil is exhausted, technical methods become poorer, etc.). New equilibrium will here be established in each case on a lowered basis, by reason of the destruction of a portion of society. We are now dealing with the declining society, a disappearing system, in other words, with motion having a negative indication.[13]

The similarities between this statement and Dr Meggers' formulation are so striking that they could well grace a *New Yorker* "strange coincidence" column! The author of the view which is paralleled by Dr Meggers is N. I. Bukharin, one

12 Meggers, 1960, pp. 302-303.

13 Bukharin, 1925 (2nd English edition), pp. 76-77.

of the inner group of Old Bolsheviks and a Marxist spokesman whom Lenin called "the most valuable and greatest theoretician of the party." Bukharin was the editor of *Pravda* from 1917 to 1929 and held high offices in Russian and international Communism until he sided with A. I. Rykov against Stalin in 1928. He lost political influence and was shorn of high offices as Stalin consolidated his position, and he finally became the principal target and victim of the Stalinist purge of 1938.[14]

In this book, *The Theory of Historical Materialism*,[15] which was for many years a standard theoretical text of communists, Bukharin returns time and again to the subject of the interrelations between energy, technology, and social evolution. His entire chapter, "The Equilibrium between Society and Nature," is an exploration of these interrelations from the Marxist point of view and, indeed, is liberally studded with quotations from Marx, who Bukharin believes anticipated the energy measure in principle. A citation or two from this interesting chapter may be in order.

> Man, as we have seen above, is constantly making use of the laws of nature in his struggle with her. "He makes use of the mechanical, physical and chemical properties of some bodies in order to make other substances subservient with his aims."[16] Man makes use of the power of steam, electricity, etc., the attraction of the earth for bodies (law of gravitation), etc. It is impossible, therefore, for the *state of nature* at a certain place and at a certain time not to act upon human society. . . . The importance of minerals and metals has increased particularly in recent days (iron, coal). . . . The centers of modern heavy industry are determined by the location of deposits of iron ore and coal. The character of the soil, together with the climate, have their influence on the vegetable kingdom. . . . It is well known how significant are the inundations of the great rivers (Nile, Ganges, etc.) for agriculture, and how great was the influence of this circumstance on the ancient Egyptians and East Indians. Water is also important as motive power (water-mills are among the earliest inventions; therefore, cities arose in close proximity to regions rich in water; more recently the utilization of water power

14 *Report of Court Proceedings in the Case of the Anti-Soviet "Bloc of Rights and Trotskyites,"* 1938.

Bukharin was a member of the Central Committee of the Russian Communist Party from 1917 to 1937. He served in the Politburo from 1923 to 1929. He was a member of the Executive Committee of the Comintern from 1919 to 1929 and the head of it for a three-year period. His dislike and distrust of Stalin were his undoing, although Stalin at one time tried to woo him with such flattery as: "You and I are the Himalayas, the rest are unimportant." In keeping with one of the tenets of historical materialism Bukharin consistently wrote of the insignificance and impotence of the individual when compared to the overwhelming march of social and historical forces. It is ironic that he lost his life because he clashed with that somewhat important and determined individual, Stalin!

15 The title of the English editions is altered to *Historical Materialism: a System of Sociology*. The text remains the same.

16 Bukharin notes that this quotation is from Marx's *Capital*, vol. 1, p. 199. Actually Marx develops this point in considerable detail on pp. 198 to 201.

in electrification may be mentioned, the so called "white coal," now widely exploited in America, Germany, Norway, Sweden and Italy). . . . Human society, ever since it began, has had to abstract material energy from external nature; without these loans it could not exist. Society best adapts itself to nature by abstracting (and appropriating to itself) more energy from nature; only by increasing this quantity of energy does society succeed in growing. . . . The metabolism between man and nature consists, as we have seen, in the transfer of material energy from external nature to society; the expenditure of human energy (production) is an extraction of energy from nature, energy which is to be added to society (distribution of products between the members of society) and *appropriated* by society (consumption); this appropriation is the basis for further expenditure, etc., the wheel of reproduction being thus constantly in motion.[17]

On the question of the primacy of the technological over the social organizational and the philosophical Meggers is an echo of Bukharin, who says:

. . . If, in the case of a certain society, we know what instruments of production it controls, how many such instruments, what kinds of workers and how many, we shall also know what will be the productivity of social labor, and what will be the degree to which this society has conquered nature, etc. In other words, the instruments of production and the working forces give us a precise material measure for the stage attained in the social evolution.

We may also glance a little deeper; we may go so far as to say that the instruments of production determine even the nature of the worker. For example, when the linotype machine is added to the system of social labor, workers will be found to run the machine. The elements acting in the labor process are therefore not merely an aggregation of persons and things, but a system in which all things and all persons stand, as it were, at their posts, having become adapted to each other. We may therefore definitely state that the system of social instruments of labor, *i.e.*, the technology of a certain society, is a precise material indicator of the relation between the society and nature. . . .[18] Technology is a varying quantity, and precisely its variations produce the changes in the relations between society and nature; technology therefore must constitute a point of departure in an analysis of social changes.[19]

Again, on this same subject, Bukharin asks, "Is it conceivable, for instance, that the technological system of society, the structure of its tools, should be along certain lines, while the structure of human relations should be along entirely different lines?"[20] And he hastens to answer in the negative. On this point, in another place, he quotes a well-known paragraph from Marx's *A Contribution to*

17 Bukharin, 1925, pp. 105-110. For other references in this volume to the importance of energy in social evolution see also pp. 89, 90, 91, 112, 114.

18 *Idem*, p. 115.

19 *Idem*, p. 121.

20 *Idem*, p. 136.

a Critique of Political Economy, which includes the sentence, "The mode of production of material life conditions the social, political, and mental life-process in general."[21] After a long discussion and many examples he summarizes by saying: "*The type of economic structure*, therefore, also determines the type of the social-political structure and of the ideological structure."[22]

On the fiftieth anniversary of the death of Karl Marx, the Soviet Academy of Science issued a memorial volume of essays by Marxist notables. Bukharin contributed a long lead article entitled "Marx's Teaching and its Historical Importance" in which he again stated the Marxist position on the point Dr Meggers discusses. He said, in part:

> There lies at the basis of the theory of historical materialism the materialist premise that all the vital wealth of society, the whole context of its complete process of life, is in the long run determined by the level of power over nature, by the degree of real mastery (and thus of real change) of the external world, i.e., by the movement and self-movement of productive forces, by the process of material labour which always takes place in a concrete historical social form, that is to say, which is continuously connected with the economic structure of society. . . . The material fact of the process of the development of productive forces (or their decline) in its social-historical form, that is the changes in the productivity of social labour and in human relations in the process of that labour (productive relations), these are the main determinants which in the last resort, either directly or indirectly, immediately or through a number of intermediate links, condition the changes in the whose sphere of superstructures, political, juridical, moral, scientific, aesthetic, philosophical.[23]

The resemblances between what Bukharin wrote in 1921 and before and what Meggers represents to be developments in anthropology in 1943 and later hold in respect to the smallest details and broadest formulations alike. Let us take, for instance, Dr Meggers contention that "cultures are composed of three general classes of phenomena: technology, social organization, and philosophy" and that "of these, technology is primary and determines the content and form of the other two components."

This may be compared with what Bukharin has to say about these same elements and their relationship:

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21 *Idem*, p. 243.

22 *Idem*, p. 239.

23 Bukharin and others, 1936, pp. 40-41. For an earlier reflection of Bukharin on the implications of Marxism for social science we have his *The Economic Theory of the Leisure Class*, written in 1914 and published in 1919, in which we find these sentences (p. 36): "The following sociological bases of economic science are characteristic of Marxism: recognition of the priority of society over the individual; recognition of the historical, temporary nature of any social structure; and finally, recognition of the dominant part played by production."

We defined society above as an aggregation of persons. In a broader sense, however, society also includes things. Present-day society, for instance, with its vast stone cities, its giant structures, its railroads, harbors, machines, houses, etc.; all these things are material technical "organs" of society. . . . .Technical devices are not merely pieces of external nature; they are extensions of society's organs; we may therefore take a broader view of society than we have thus far done; we may make it include also things, *i.e.*, society's technical apparatus, its system of working devices. . . .

We have seen in chapter iv that society constitutes a system of *persons* considered together; now we see that *things* must also be so considered. . . . But we know very well that human beings are not merely physical bodies, they think, feel, wish, pursue goals and are constantly changing in their thoughts and desires. The relations between persons are not only material working relations, but also psychical relations, "mental" relations; society produces not only material objects; it also produces the so called "cultural values": art, science, etc.; in other words, it produces *ideas* in addition to things. These ideas, once they have been produced, may be developed into *large systems of ideas*.

The trinity of elements in society therefore includes: things, persons, ideas. . . .

We have already pointed out that in a consideration of social phenomena it is necessary to begin with the social, material productive forces, with the social technology, the system of tools of labor. . . .[24]

It is obvious from this statement and from the many examples following it that the terms which Dr Meggers uses in her formulation (technology, social organization, philosophy) are synonyms for the concepts (things, persons, ideas) through whose interrelations Bukharin traces change in societies. In various ways, with a wealth of illustrative material, Bukharin presents the argument that Meggers is to rediscover decades later. One of the most succinct statements of his formula and one upon which Dr Meggers has not improved is this: "The technology conditions the mode of production; the mode of production conditions the view of life; this chain uniting the material, human, and mental system creates a certain type of society."[25] Nor in spite of the new chronology Dr Meggers has given us, can I find that she has in principle gone beyond a Bukharin summary that reads: "Thus, the system of the superstructure, from the social-political to the philosophical phase inclusive, is connected with the economic basis and the technical system of the specific society, being a necessary link in the chain of social phenomena."[26] Note that Bukharin applies his formula to *the specific society*. So much for Dr Meggers' claim that it was left to her in 1960 to suggest how evolutionary theory "can be used as a guide in understanding the dynamics of

24 Bukharin, 1925, pp. 132-134.

25 *Idem*, p. 233. For other expressions by Bukharin of this view see pp. 117, 120, 121, 123-124, 137, 138, 142, 143, 150, 154, 161 in the same volume.

26 *Idem*, p. 225.

individual cultures." Bukharin's book is peppered with explanations and interpretations, flowing from the formula, of individual situations and cultures. Apparently the "practical tool kit" Dr Meggers urges upon "the field anthropologist" is not quite so new as she represents, and its main contents seem to be a somewhat shopworn hammer and sickle. If there is any doubt of this, the 1960 Meggers model may be compared with this pithy Bukharin resumé of 1921:

> This thesis is irrefutable; it follows that our analysis must begin with the productive forces, that the countless mutual dependences between the various parts of society do not eliminate the *basic, ultimate* dependence of all social phenomena on the evolution of the productive forces; that the diversity of the causes operating in society does not contradict the existence of *a single unified casual relation in social evolution.*[27]

There is one striking difference between Bukharin and Meggers, though it is not a matter of theory. Bukharin, in talking about the triad of technology, social organization, and ideology and the determining and molding role of the first of these on the others, does not claim priority and originality for himself. Instead he modestly suggests that Marx said it first and refers to or quotes passages from *Capital,* such as this one, which certainly seems to bear him out:

> Darwin has interested us in the history of Nature's Technology, *i.e.,* in the formation of the organs of plants and animals, which organs serve as instruments of production for sustaining life. Does not the history of the productive organs of man, of organs that are the material basis of all social organization, deserve equal attention? . . . Technology discloses man's mode of dealing with Nature, the process of production by which he sustains his life, and thereby also lays bare the mode of formation of his social relations, and of the mental conceptions that flow from them.[28]

27 *Idem,* p. 229.

28 Marx, *Capital,* vol. 1, 1915, p. 406, footnote. Quoted in Bukharin, 1925, p. 116. The entire section (Chapter XV, Section 1. "The Development of Machinery," pp. 405-422) from which Bukharin draws this quotation should be carefully read, for in it Marx discusses "energy" (which he refers to as "motive power") and indicates his conception of its relationship to technology and cultural evolution. The closeness of his view to that espoused by Dr Meggers can be gleaned from this fragment: "The machine, which is the starting point of the industrial revolution, supersedes the workman, who handles a single tool, by a mechanism operating with a number of similar tools, and set in motion by a single motive power, whatever the form of that power may be. Here we have the machine, but only as an elementary factor of production by machinery. . . . Not till the invention of Watt's second and so called double-acting steam-engine, was a prime mover found, that begot its own force by the consumption of coal and water, whose power was entirely under man's control, that was mobile and a means of locomotion, that was urban and not, like the water-wheel, rural, that permitted production to be concentrated in towns instead of, like the water-wheels, being scattered up and down the country, that was of universal technical application, and, relatively speaking, little affected in its choice of residence by local circumstances" (pp. 410-412). Attention is again called to Marx's statement in his preface to *A Contribution to*

I have quoted from Bukharin at some length because, unlike some Marxist theorists, he does not pass over questions of the source and amount of energy a culture utilizes by general reference to "the productive forces" and "man's increasing control over nature" but, like Dr Meggers, specifically distinguishes between the energy harnessed and the technical means of directing it and, like her, takes both factors into account in his conception of cultural evolution.

Yet Bukharin was by no means the first to do this. In 1908 a book of essays by seven well-known left-wing leaders was published in St. Petersburg under the title, *Outlines of Marxian Philosophy*.[29] The concluding piece in the volume was "Foundations of Social Philosophy" by S. Suvorov. In what he wrote Suvorov was reflecting the interest of a group of Marxist intellectuals, led by A. Bogdanov, in the "energetics" of Wilhelm Ostwald, the noted German chemist, whose writings on the philosophy of science had attracted considerable attention.[30]

Suvorov wrote of a "law of economy of forces" which "appears as a unifying and regulating principle of every development—of the inorganic, biological and social." The substance of his law, according to him, "is that *the less expenditure there is in each system of forces, the more adapted it is for conservation and development; the greater its accumulation becomes and the more effectively is that accumulation served by expenditure.*"[31] The solar system, the cycle of earthly phenomena, and the process of life, he explains, "are produced and developed because of the saving and accumulation of energy which belong to them in virtue of their inner economy."

In the domain of the social, according to Suvorov, the "universal Law" is interpreted to mean "the evolution of productive forces," an evolution which "appears

the Critique of Political Economy: "The mode of production in material life determines the social, political and intellectual life process in general." Referring to this prefatory statement, Lenin asserts in his *The Teaching of Karl Marx* (p. 15), "Marx gives an integral formulation of the fundamental principles of materialism as applied to human society and its history. . . ." Stalin, in writing of the same material, says in his *Dialectical and Historical Materialism* (p. 44): "Here is the brilliant formulation of the essence of historical materialism given by Marx in 1859 in his historic Preface to his famous book, *A Contribution to the Critique of Political Economy*." On his own account, Stalin writes in his book at page 29: "Whatever is the mode of production of a society, such in the main is the society itself, its ideas and theories, its political views and institutions." I am indebted to Dr Robert Ascher for calling my attention to these statements of Lenin and Stalin.

29 Bazarov and others, 1908.

30 Ostwald, 1902. Bogdanov was the pseudonym used before the revolution by A. A. Malinovsky (1873-1928), Russian philosopher and economist.

31 Bazarov and others, 1908, p. 293. From the context it is quite evident that Suvorov considers the *least* waste or *unnecessary* expenditure of energy to be an aid to conservation and development.

as the determining principle of all economic and social development." The development of the productive forces, the author further tells us, "corresponds to the growth of the productivity of labour, to relative reduction in expenditure and to an increase in the accumulation of energy." "In general the productive forces of people . . . consist of labour energy, of harnessed elemental forces of nature, and of a nature which has been modified by culture and the tools of labour, or productive technique." Evolutionary advance results from an accumulation of energy and an increase in the productivity of its expenditure by an application of more advanced technology. There is a concomitant reduction of labor or manual expenditure of energy.[32]

Not only did "energy" figure prominently in Marxist evolutionary doctrine of the early twentieth century, but its proper place in materialist theory became a focus of controversy among socialists. Marx and Engels had reacted violently against German idealism and had replaced mind by matter as the central concept and absolute reality of their philosophical system. Their materialism seemed secure as long as one substance and form after another could be explained as some kind of arrangement of particles of matter. But when, as a result of the teachings of the new chemistry and the new physics, substances which had confidently been described as organizations of matter were increasingly referred to in terms of electricity and "waves," many intellectuals feared that the foundations of materialism were crumbling. Prominent physicists were writing that the atom was dematerializing and matter was disappearing. Philosophic idealists were asking sarcastically whether it was still believed that science and materialism supported each other.[33] Some Marxists feared that their key concept was dissolving before their eyes and that their ideological house would tumble about their ears. Both Marx and Engels knew that the materialism they propounded, permeated as it was with the science of the day, had constantly to be reconciled with developments in science if it were to resist attack successfully. Marx kept a sharp eye out for scientific data that had relevance for materialist doctrine and continually accumulated notes for use in defense of materialism. Engels, who inherited these materials, planned and began a substantial treatise on the relationship between the new science and socialism and doubtless would have completed it if Marx's death had not burdened him with the editing and finishing of the last two volumes of *Capital*. The notes and fragments which were to have been incorporated into this volume were finally published in 1927 under the title, *Dialectics of Nature*.

32 Quoted and summarized in Lenin, 1909, pp. 288-289.

33 *Idem*, pp. 218-219. To appreciate the full impact of the new scientific discoveries on philosophical materialism, all of Chapter V ("The Latest Revolution in Natural Science and Philosophic Idealism"), pp. 211-269, should be read.

Actually, it was Lenin who became so exercised over the threat to materialism of "the revolution in natural science" and so incensed by what he interpreted to be unwise and unnecessary concessions to idealism on the part of panicky or befuddled Marxists that he took the time to write a book which described and interpreted the science of his day and which asserted that there was no incompatibility between it and the strictest interpretation of materialism. Throughout this book Lenin roundly scolded "revisionists" and "Machians" and those who sought escape from materialist dilemmas by a strategic retreat from the classical terminology of Marx and Engels. This work, of course, is Lenin's *Materialism and Empirio-Criticism,* published in 1909.[34]

The background of Lenin's ire is instructive. Wilhelm Ostwald in his *Lectures on the Philosophy of Nature* had suggested that "the old problem of how to connect the conceptions of matter and spirit" could be resolved by subsuming both under the term "energy." A number of Lenin's Marxist comrades, feeling that "matter" was becoming a dubious ideological asset and that a term proffered by a distinguished natural scientist would make a most honorable and acceptable substitute, looked with favor on the suggestion. Lenin was convinced that they had fallen into an idealistic trap and that their search for a new terminology betrayed an ignorance of the fundamentals of materialism. He was "enraged," to use his own term, that "matter" and "mind" or "spirit" should be treated as coördinates and that an attempt should be made to expunge any distinction between them by absorbing them into a single "broader" concept. For Lenin the issue between materialism and idealism was not whether matter was to be conceived of as atoms, other particles, electrical charges, or waves in motion. It was whether matter exists prior to mind and is anterior to mind. On this point he declared himself in the strongest terms:

> For the sole "property" of matter—with the recognition of which materialism is vitally connected—is the property of being *objective reality,* of existing outside of our cognition. . . . To put the question from the only correct, that is, the dialectico-materialistic standpoint, we must ask: do electrons, ether, etc., exist as objective realities outside of the human mind? The scientists must answer this question without hesitation and the answer must be an affirmative one, for they recognise without hesitation the

34 *Idem,* p. 5. Ernst Mach, the Austrian physicist and philosopher, was deemed by Lenin to have had special responsibility for influencing Marxists toward idealism. Prominent among those whom Lenin felt had been contaminated by Machism were V. Bazarov, A. Bogdanov, V. Chernov, Maxim Gorky, Karl Kautsky, A. Lunacharsky, S. Suvorov, N. Valentinov, and P. Yushkevich. Mach's influence extended far beyond the Marxists, however. Wilhelm Ostwald, Bertrand Russell, Paul Carus, and many others have acknowledged a debt to him. He is credited with stimulating the growth of pragmatism in philosophy, and, in spite of Lenin's dissatisfaction with him, he was considered an opponent of metaphysical thinking in science.

existence of nature prior to man and organic matter. . . . The electron is as inexhaustible as the atom, nature is infinite, but it *exists* infinitely; and only this categorical, unconditional recognition of its existence beyond the consciousness and sensation of man, distinguishes dialectic materialism from relativist agnosticism and idealism.[35]

It is apparent that Lenin was here taking a "hold the line" position and was anything but cordial to revision or expansion of the Marx-Engels vocabulary or conceptual system. To Lenin, Ostwald was "a very great chemist and very confused philosopher."[36] In his eyes, "the fashionable 'energetics' of Ostwald" was "a muddled agnosticism, with leanings toward idealism."[37] He heaped scorn on Marxists such as Bogdanov and Suvorov who experimented with unorthodox labels. It is certain that Lenin's indignation over the faintheartedness of some of his fellow Marxists and his call for strict adherence to Marx-Engels terminology prevented as extensive a use of the energy concept as otherwise would have occurred. And, of course, Lenin's growing authority and ultimate political power made it hazardous for Marxist intellectuals to persist in modes of expression against which he had taken such a determined stand. Indeed, the wonder is that prominent Marxists like Bukharin dared to discuss "energy" and its relation to cultural evolution so freely.

Obviously a doctrine which drew so handily from a conception of "man's increasing control over nature" could not entirely ignore the harnessing of new sources of energy; and in point of fact, without employing the controversial labels, even the most disciplined and orthodox Marxists continued to discuss such developments and their implications. For instance, the formulations of George Plekhanov, who stood firmly with Lenin in this doctrinal and terminological tug of war, do not differ in substance from those of Bukharin.[38] Plekhanov adroitly manages to convey his conception of the effects of new sources of energy by references to "expansion of the productive forces," "emancipation from nature's yoke," "the measure of the authority of man over nature," and similar phrases. Nor need we stop with Plekhanov. The materials I have reviewed in this paper constitute

35 *Idem*, 1909, pp. 220-222. Scattered throughout the book are a large number of similar statements.

36 *Idem*, p. 136.

37 *Idem*, p. 194. For a summary account of the life and ideas of Ostwald see the chapter devoted to him in Slosson, 1914. Ostwald had a very clear conception of the importance of technology and the tapping of new sources of energy to culture and cultural development. He felt, however, that intelligence and good will were needed to guide the use of scientific discoveries and and he tried to work into his formulae the consequences of beneficent or baneful employment of new energy resources.

38 Plekhanov, 1940; 1947. A good deal of Plekhanov's work was first published under the pseudonym of N. Beltov.

the merest introduction to the many interesting resemblances between Dr Meggers' writings and what is to be found in the books and tracts of the Marxist theorists. In this connection, one could also consult the works of the Italian Marxist, Antonio Labriola, and any number of others.[39]

I began with a critique of the application of a particular theory to a certain situation and body of data. The theory asserts that culture is a consonant whole in which social organization and philosophy are completely molded by technology and only undergo the kind and amount of change that alteration in technology dictates. I found that the theory did not apply. I have felt it necessary to probe the historical roots and affiliations of the theory a bit more thoroughly than Dr Meggers has seen fit to do. It seems incredible that anyone seeking to place in time and in the history of theory a point of view which systematically subordinates the social and ideological to the technological should completely overlook Marx and Engels and their heirs. It is curious that our neo-evolutionists constantly acknowledge their debt to Darwin, Tylor, and Morgan and never have a word to say about the relation of their ideas to those of Marx, Engels, Bukharin, Plekhanov, Labriola, Suvorov, Lenin, Stalin, *et al.* Yet it is patent that their formulations are a great deal closer to those of Bukharin and, indeed, to those of any thoroughgoing historical materialist than they can ever be to those of Tylor or Morgan. What they share with Tylor and Morgan is a conviction that cultural evolution has taken place. With Marx, Engels, Bukharin, Plekhanov, Labriola, and the others they share, in addition, convictions concerning the elements and mechanisms through which the process occurs. This close correspondence may be an instance of parallelism. It may be a result of stimulus diffusion. There is the possibility of lineal or collateral descent. It could reflect still other dynamic forces. It might be helpful to know what is involved. At any rate, before it becomes a matter of total assimilation, anthropologists might well take a closer look at recent intellectual history and at some recent trends in American ethnology.

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39 Labriola, 1904; 1907. Labriola was not unaware of the implications of the "extensive application of the principle of energy," as he termed it. See Labriola, 1907, p. 97.

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INVENTION VERSUS DIFFUSION: THE BURIAL MOUND COMPLEX OF THE EASTERN UNITED STATES

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THE CONCEPT of an Asiatic origin for the so-called Woodland ceramic tradition of the eastern United States has become widely accepted, and certainly there is an accumulating body of evidence tending to support it. It would seem only logical, therefore, that associated cultural features which also appear suddenly at this time in prehistoric North America without apparent local antecedents should be ascribed to the same source—and in particular that most conspicuous of all Woodland elements, the burial mound with its accompanying mortuary complex. This was first proposed by implication twenty-three years ago by McKern. It was asserted quite strongly by Ford in 1955 in the Jaketown report, and indicated as a serious possibility by Willey and Phillips in their famous synthesis that same year—since reiterated in the new edition. Now any hypothesis dignified by such auspices is bound to gain currency and to be repeated by others, the most recent instances being Rainey (in a paper in Moscow, USSR), Beals and Hoijer (in their popular text *An Introduction to Anthropology*) and Wormington (in the new edition of her authoritative handbook on early man in America). In every case, the burial mound hypothesis is linked with the already-accepted thesis of an Asiatic origin for the ceramic tradition, and thus acquires by sheer contagion a sort of spurious authenticity in the mind of the reader. It seems likely that what was once a mere suggestion will, through sheer repetition, become entrenched as dogma. And I submit that an erroneous hypothesis once entrenched can seriously hinder the ultimate solution of major problems.

Now I should make it clear at the outset that I am not opposed to the idea of Asiatic origins for specific New World features. Far from it. But any hypothesis should be supported by some evidence, and this one has never been. It stems from the days when Siberia was a vast unknown—a sort of *deus ex machina*, ever ready to rescue the American scholar from his historical dilemmas. But those days of easy theorizing, unfortunately, are over. We are beginning to know too much about Siberia. It is high time, therefore, to take a look at this hypothesis for Asiatic origin of the eastern North American burial mound complex in the light of the known facts.

This complex had its first major efflorescence in the famous Adena culture of the Ohio valley, which was in full blast by about 800 BC according to the

accumulating radiocarbon dates. It was certainly present in this area by 1000 BC and possibly as early as 1500 BC. The associated Woodland ceramic tradition is at least as old and is thought to have diffused from northeastern Asia sometime during the second millennium, since the necessary elements were present in the forest zone of Siberia by, or soon after, 2000 BC. But was the burial mound complex also present there and thus available for a parallel diffusion? The answer to that is a flat NO. Burial mounds are unknown in the Siberian forest at any period. We can be quite unequivocal about this, since so conspicuous a feature could scarcely have escaped notice even in areas which have not been subject to actual archaeological exploration.

Before we ask whether some other region of northern or even central Eurasia could not have provided the source, let us briefly survey the history, distribution, and nature of burial mounds (or kurgans, as they are generally called) in this part of the Old World. The first kurgans anywhere in this area appear in the northern Caucasus during the local Copper Age, which occupied the second half of the third millenium BC (roughly 2500 to 2000). From here the practice apparently spread both westward and eastward along the steppe zone. In Asia, this diffusion came to a halt for a long time in the general region of the Altai Mountains, not quite half way across the continent. It is associated with western-oriented cultures whose bearers were people of European type, and these seem to have reached their frontier at this point. Siberia was affected by these developments only along its southern margin of grassland, and here only west of the Yenisei River. Initially, the burial mounds seem to appear in a very attenuated form. The first suggestion of kurgans in this part of Siberia dates from the later phases of the copper-age Afanasievo culture (probably after 1700 BC)—but these are of miniature size. The main feature of all Afanasievo burials is a ring of vertical stone slabs on the surface; the small mound, where it occurs, strikes one as an afterthought. The use of kurgans is encountered sporadically in the succeeding Bronze Age Andronovo culture, but here again a stone enclosure is the characteristic feature and obviously the center of attention. The first full-sized kurgans seem to appear in western Siberia only in late Andronovo times—1200 or even 1000 BC—and we do not find them for certain farther east in Siberia before 700 BC. At that time they are associated in the Altai Mountains with the Maiemir culture—the oldest nomad culture in this area—and in the Minusinsk Basin on the upper Yenisei with the Tagar culture (or Minusinsk Kurgan Culture, as it used to be called). Both are late Bronze Age. A characteristic feature of nearly all these large kurgans is the use of stone in their construction: sometimes mixed with earth, but more often forming a solid covering. The famous Pazyryk tombs with their

wealth of perishable material preserved frozen in ice are of this type. In form they are like an inverted saucer: extensive in ground plan but very low, only a few meters in height. The contrast with the tall, conical earth mounds of the Adena culture is obvious. Not until about the first century BC in the final stage of the Tagar culture do you find hemispherical earth mounds that recall in any way those of eastern America, and none of these exceed four and one-half meters in height. We should point out also that in every case these steppe kurgans of Asia cover a sub-surface grave; the burial chamber is never within the mound itself. Nor is there any evidence of anything that could be labelled a mortuary cult, with which the American mounds are characteristically associated. Any parallels in burial practices are too universal and generalized to have any possible significance.

All during this time—nearly two thousand years—the custom of burial mounds never spread either northward into the forest zone or eastward onto the steppe beyond the Altai region. Apparently it was not until about the beginning of the Christian era that the practice was adopted in Mongolia. Here the first examples seem to be the famous Noin Ula tombs which have been linked with the Huns. Thereafter, the idea seems to have spread rapidly to the Pacific, since the first Japanese tumuli (which are Early Iron Age and dated no earlier than 250 AD) can probably be ascribed to a steppe nomad source. The appearance of the trait anywhere in north*east*ern Asia is thus very late, despite its long history in the west: far too late to have affected the New World.

What I wish to emphasize, however, is that the use of burial mounds is a typically steppe trait; that it is always associated with metal; and that in Siberia it seems definitely connected with a herding life and especially with pastoral nomadism. From a purely theoretical point of view one might debate whether specifically steppe traits could in fact diffuse northward over the great distances required and through an alien environment. I would doubt it, but in any event the question seems unnecessary in view of the absence of specific parallels between any of these steppe cultures and American ones. For New World analogies in Asia at this period we must look in the forests and on the sea coasts.

There is one occurrence of burial mounds farther south in eastern Asia that requires mention for its possible significance, and that is China. Here, they were first introduced by the Chou about 1027 BC and possibly slightly before, as typified by the imposing tumuli in the Wei River valley. They are not present in Shang or earlier periods. These Chou mounds therefore do not antedate the American complex—or at least not sufficiently to enter the picture. In actual fact, this Chinese use of burial mounds did not even spread to neighboring Japan for

over 1200 years, if at all, since the Japanese mounds are very probably not of Chinese inspiration.

The only reason for dwelling any further on the Chou tumuli is the possibility that they might have stemmed from some *earlier* non-Chinese focus of mound-building in eastern Asia outside the North China area—old enough to conceivably have had some relevance for New World problems. We must not make the mistake of assuming that Siberia is the only imaginable hearth for American cultures, overlooking the possible contributions of pre-Chinese and non-Chinese traditions in eastern Asia.

Though it seems doubtful that any such older focus could have escaped detection by now, this cannot be considered a conclusive argument in the present state of Chinese archaeology. I would prefer to take another approach: namely, that the Chou were themselves western barbarians who, if not actually a steppe people, were undoubtedly in close contact with the latter and thus open to new ideas from the west. We know that the practice of kurgan burial was well established by then on the western steppes, and that it was spreading eastward, although in the north its progress was arrested in the Altai region. There may have been no such interruption to its diffusion farther south, in which case the custom could easily have penetrated to western China before the rise of the Chou dynasty. Certainly there is abundant evidence of continual cultural exchange with China along this southern route through the Tien Shan all during this period, which at least shows that the way was open. Although the Chou mounds have not been excavated as yet (to my knowledge), the known Chou-period tombs would fit into the general steppe tradition and certainly do not suggest an entirely different complex. We may assume that the reluctance of the Mongolian population to adopt kurgans from their Altai neighbors until a late date would also have prevented the diffusion of mound-building up into northeastern Asia via this southern route.

We may sum all this up by saying that burial mounds were not in use in northeastern Asia at an early enough date to have been ancestral to the New World complex; that they are closely linked with the steppes; and that they never got any nearer to Bering Strait than Japan. They bear little resemblance to the American examples and are not associated, in the Old World, with any traditions that have American analogies—least of all, with the Asiatic roots of the Woodland ceramics.

This would seem to be a case in which New World archaeologists will have to dig for the answers in their own backyard instead of awaiting rescue from abroad. And in this connection, recent discoveries by Ritchie offer a possible clue. Ritchie has unearthed evidence suggesting the existence of an Early Woodland burial cult in the Northeast, at least some of its roots being traceable back into late Archaic

times. This appears to have been a cult idea, not a culture. Like the much later Southern Cult, it diffused into many cultures. Ritchie believes that this cult developed into the elaborate subsequent complexes like Adena and Hopewell. It is characterized by cremation and the burial of remains in mound-like natural knolls. The latter custom might, of course, explain the later addiction to artificial mounds.

Be this as it may, we can at least say with certainty that the burial-mound complex of the eastern United States represents an independent development that owes nothing to the Old World.

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VARIETIES OF CHANGE IN KORIKI CULTURE[1]

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IT NOW SEEMS CLEAR that in recent years anthropology has been developing a more generalizing attitude toward culture change than it has had for a number of generations. With a few notable but individual exceptions the reactions against nineteenth century social evolutionism tended to crystallize around either a position which saw change as irrelevant to the essential understanding of culture or one which regarded change as a phenomenon so pervasive as to make each instance completely unique. Both emphasized research, and over the years, as anthropologists on one side or the other worked out the patterns of knowledge which came from their separate assumptions, much was accomplished and the contributions to anthropology were great.

As a period of scientific development, however, it was also characterized by a tendency to isolate individuals who had an interest in finding general patterns in social change or to keep their constructions on such low levels of abstraction that they were incapable of properly separating the general and relevant from the unique and irrelevant. The isolating process was not one which stopped the flow of words but only that of meaning. The basic assumptions brought to the debate by those who saw change as either unimportant or a matter of individual culture history were so different from those who saw it as something important and with understandable patterns that the arguments, after the initial statements of positions, were inclined to become repetitious.

The diffusion versus indepedent invention argument is an example of a part of the larger debate. Seen as a phase in anthropological history, the discussions which revolved about that question seem to occupy a place in the development of our discipline that is similar to the one that the famous nature-nurture controversy has found in the history of psychology. Both stimulated a great amount of argument and inquiry. But both were essentially meaningless questions in the forms in which they were posed, and their separate controversies contributed to science more in spite of them than because of them.

Today there is a greater inclination to see that diffusion and invention are, as Malinowski[2] essentially said, the two sides of one coin. In addition, many con-

1 The research upon which this article is based was financed by a grant from the Ford Foundation.

2 Bronislaw Malinowski, *A Scientific Theory of Culture* (Chapel Hill: University of North Carolina Press, 1944). It would also seem fair to say that Steward's recognition of the fundamental unity of diffusion and invention was an essential prerequisite to his important article on

temporary anthropologists clearly believe that in some way or another the concept of evolution has an appropriate and necessary application to cultural data. There is, however, no single-minded uniformity to present thinking about social change. It is quite clear that there are important differences between the views of such men as Eggan, Murdock, Steward, and White, to name a few of the more prominent anthropologists who are interested in the subject. Each of them has at one time or another made some of these differences quite explicit. But they and others share at least the basic assumption that statements about change that have a general application can be abstracted from the study of empirical cases. They also apparently regard this as one of modern anthropology's important tasks. Within this climate the debate of differences can be fruitful. When points of view rest upon basically different assumptions, argument between them can serve to develop the possibilities of each position separately, but little can be expected in the way of new organizations or syntheses, at least from the argument itself. But with the present tendency to agree on some basic ideas and objectives, the developing debate between the real differences that exist can contribute much to the evolution of social theory.

But regardless of differences in theory, most approaches to culture change of a generalizing nature become involved with the construction of types as a useful stage in the process of abstracting and generalizing. In most cases such types are one of two sorts. Steward's[3] analysis of the rise of higher civilizations produced a temporal succession of cultural types. Murdock[4] has done essentially the same thing in his studies of social structure in that his typological emphasis is upon cultural systems. The other phenomenon in studies of culture change which has moved students to the abstracting process of typology has been change itself or the processes by which change occurs. To speak of only a few examples, Barnett[5] has presented what are essentially types of innovative processes, and Vogt[6] has spoken of "recurrent" and "directional" processes. Perhaps the best known example of an effort to construct a type of change or process is Childe's[7] work with the concept of social revolution.

Cultural Causality and Law: a Trial Formulation of the Development of Early Civilizations (American Anthropologist, vol. 51, pp. 1-27, 1949).

3 Steward, *op. cit.*

4 George P. Murdock, *Social Structure* (New York: Macmillan Co., 1949).

5 H. C. Barnett, *Innovation: the Basis of Cultural Change* (New York: McGraw-Hill Book Co., Inc., 1953).

6 Evon Z. Vogt, *Modern Homesteaders: the Life of a Twentieth-Century Frontier Community* (Cambridge: Belknap Press of Harvard University Press, 1955), and Vogt, *On the Concepts of Structure and Process in Cultural Anthropology* (American Anthropologist, vol. 62, pp. 18-35, 1960).

7 V. Gordon Childe, *Man Makes Himself* (London: Watts & Co., 1941).

What follows here is an empirical case study which attempts to make some contribution to functional-historical analysis. In this general sense, it draws some inspiration from the work of Eggan. On a more specific level it attempts to employ some of Murdock's types of social organization and Vogt's varieties of process as both tools with which to analyze and interpret a body of empirical data and as concepts to be tested by those data. The culture concerned is that of the Koriki tribe of the delta of the Purari River in the Territory of Papua.

KORIKI CULTURE AT THE TIME OF EUROPEAN CONTACT[8]

At the time of European contact the character of Koriki life was strongly conditioned by a swamp ecology which furnished the basis for a sago-collecting economy and water transportation and by a belief and ceremonial system which encouraged cannibalism and raiding. The sago palm furnished an abundant year round staple and the stream network of the swamp offered an easy means of transporting sago logs from the place of cutting to nearly any desired processing center. The combination of a reliable food supply and an effective means of transporting it allowed the Koriki a rather broad range of possible settlement patterns. So far as the subsistence system was concerned, the Koriki could live in dispersed family units or in concentrated village groups. The fact that it was the latter that held can be seen as an effect of the requirements of ceremony and raiding. Most religious ceremonies were carried out as village responsibilities, and the more important events required cannibal victims from other tribes. The raiding and counter-raiding activities made necessary by the religious institutions encouraged large, concentrated settlements as desirable for purposes of defense and the easy muster of large war parties. As a consequence, the Koriki were gathered into one village group of uncommon size for Papua and two smaller but still substantial subsidiary groups. The larger village group, Ukiaravi, seems to have had more than 2500 residents at the time of contact, while the other two groups, Kairu'u and Koropenairu, both exceeded 600.

While the village was the basic unit in the activities of ceremony and raiding, the individual found his personal relationship to these institutions through his position in the kinship structure. The main elements in the system were patrisibs in a moiety division. Both sib and moiety were exogamous. In addition, one could not marry a person from one's mother's sib. Marriage restrictions, in other words, were bilateral. Cousin terminology was of the Hawaiian type with bifurcate

8 For a more complete description of Koriki culture as it was not long after Europeans first encountered it, see F. E. Williams, *The Natives of the Purari Delta* (Port Moresby: Anthropological Report No. 5, Territory of Papua, 1924).

merging terms for aunts[9] and uncles. Typologically, it was what Murdock[10] has called Normal Guinea. In his more recent series of social organizational types, the Koriki system at the time of contact would fall within type 5.[11]

In practice, residence tended toward a patrilocal rule. When marriage crossed villiage lines, the wife in nearly all cases went to live in her husband's village. Ordinarily, however, marriages were between individuals from the same village. When the newly married couple joined an already existing household, it was usually that of one of the husband's paternal relatives, although some instances of matrilocal residence did occur. The ideal situation, however, when the marriage was within a village, was that of the independent household and, if possible, a polygynous one. But this was an ideal which the majority failed to achieve. The facts of the sex ratio of the population allowed only a minority of men more than one wife, and the difficulties of house-building and the relatively short life of some structures virtually assured that most married couples would be living with relatives or have relatives living with them.

In sum, Koriki society was organized territorially in concentrated villages and village groups. Relationships within and between villages were structured through a kinship system of patrisibs and moieties which provided the internal organization for village raiding and ceremonial activities and set the rules of proper marriage. The concentrated settlement pattern, which produced a high rate of social interaction and gave strength to the sib system, was a product of a rich sago-collecting subsistence pattern, water transportation, and the practice of raiding and cannibalism.

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9 Williams recorded the same term, *ai*, for mother, mother's sister and father's sister. I found in my own work in 1954-55, however, that a distinction was made between the mother and mother's sister on the one hand and the father's sister on the other. The former were called *ai*, as Williams reported, but the father's sister was referred to as *aimari*. It is possible that this separation developed in the years since Williams did his field work in 1922, but I feel rather strongly that it did not. Even when collateral distinctions are present in Koriki kinship terminology, they are weak in day-to-day usage, while generation distinctions are strong. Williams reported the same situation as his experience. Given this it would have been particularly easy to miss the separate term for paternal aunt. A more general question arises here, however, of the possibility of important aspects of a system of kinship terminology being obscured by the frequent recording of only the presence or absence of particular classifications. This method of presentation would not reveal the generation emphasis-collateral de-emphasis which appears in Koriki daily usage, nor would it show a similar tendency to be more concerned, when collateral distinctions are actually made, with male relatives, whether paternal or maternal, than with female relatives.

10 Murdock, *op. cit.*

11 George P. Murdock, "Evolution in Social Organization" (in *Evolution and Anthropology: a Centennial Appraisal*, Betty J. Meggers, Editor, Washington: Anthropological Society of Washington, 1959), pp. 126-143.

KORIKI CULTURE TODAY[12]

The most important influences toward change which came out of the contact situation were the suppression of raiding and cannibalism by the Australian Administration and the individual Koriki's experience as a wage laborer for various European organizations outside of the tribal territory. The former effectively unravelled the complex of institutions which had previously characterized Koriki life, and the latter provided models for new behavior.

Given the interdependence between the institutions of raiding, cannibalism, and ceremony, and the structures of kinship and village through which they were carried out, the suppression of the first produced processes toward the dissolution of the others. Without raiding cannibalism was not possible in the Koriki frame of reference. And without cannibalism the more fundamental of the community religious practices could not achieve their objectives. The loss of these functions and the connected change in general cultural environment led to a number of basic alterations in social structure. These are alterations which are still in process, but they have gone a considerable distance, and their direction is clear. All that remains of the ceremonial system are those rituals which are primarily individual or family concerns. These mark such things as the birth of a child, the building of a new house, or a marriage. But the village ceremonies which involved the secrets of the ceremonial houses and related the sibs directly to the supernatural through offerings of cannibal victims have been abandoned along with the hereditary priest statuses. With them has gone one of the cohesive forces behind concentrated settlements.

By 1954 the sib-moiety system had also disappeared. In its place a kindred organization was developing, spreading along the axis of the old custom of bilateral exogamy, but with somewhat narrower limits of membership than the old sibs allowed. The kinship terminology has remained unchanged.

With the forces of raiding and ceremony gone, a deconcentration process has been under way in the settlement pattern. Village groups have split into separate villages, which have moved to new sites. Perhaps even more important, individuals and single families have frequently disassociated from villages and moved to their own sago grounds. Virtually no one any longer lives permanently in a village and some no longer spend any time there at all. The effect has been to weaken the old patrilocal residence tendency. When people are in the village the old pattern holds very much as it did before, but as they move to the scattered sago grounds,

12 For a more complete description and analysis of the changes brought about in Koriki culture by the contact experience see Robert F. Maher, *New Men of Papua: a Study in Culture Change* (Madison: University of Wisconsin Press, 1961).

actual practice produces a neolocal pattern,[13] even though it may not be formalized in anyone's mind. It was probably the *de facto* change in residence rules which presented the final obstacle to the continuance of a patrilineal system which had already lost many of its important functions.

Within the tribal territory the subsistence pattern remains much as it used to be. But its larger nature has been changed in important ways by the addition of wage labor for Europeans in other areas. It has virtually become a part of the individual male's life cycle to spend a year or more working for Europeans. Absences from the tribal territory have been so great as to be a major contributing factor in a rather severe depopulation process. Other results have been the acquisition of European trade tools and the development of ambitions to participate in European economic life as entrepreneurs. The former has had its main influence as an incentive to wage labor, but the latter is at the root of much that is important in contemporary Koriki activity. Conscious and organized efforts to develop production for European markets have become increasingly frequent. All of these, including the widespread and important Tommy Kabu Movement,[14] have failed in their economic objectives. Each, however, has added to experience, and it seems clear that the future will bring more radical changes in the Koriki economic system.

What has been occurring in Koriki culture represents a fundamental alteration in social organization. In Murdock's terms the shift has been from Normal Guinea to Hawaiian or from type 5 to type 2. The processes which have been running through Koriki social life as a result of the contact experiences are producing a type of change which involves the destruction of old cultural patterns and the organization of new ones. It is the variety of change which is associated with the emergence of one type of cultural system from another.

PRE-CONTACT CHANGE IN KORIKI CULTURE

The ethnographic literature on culture change is dominated by analyses, in various forms, of the type of change which has been presented above. That is, a process which produces a radical reorganization and frequently a disorganization in a culture. Some of the reasons for such a bias are reasonably clear. Anthropology

13 Since ownership of sago grounds is in the paternal line, it might be argued that what has been described here is still a patrilocal situation. Technically this might be true, but for purposes of social interaction it is not. Any individual family's sago holdings tend to be scattered, and families set up their households more or less where they wish and with whom they wish, with the one condition that the sago places be relatively near.

14 Robert F. Maher, *Tommy Kabu Movement of the Purari Delta* (Oceania, vol. 29, pp. 75-90, 1958).

has developed during a period of history which has been characterized by the impact of a multitude of culture contact situations, which have been the overwhelmingly important experience in the lives of most primitive societies at the time anthropologists came to know them. As a result, a great proportion of culture change studies have been concerned with some aspect of the acculturation process as it is felt by a weaker society under great pressure from a stronger one. In addition, anthropologists who have attempted to describe and analyze native cultures as they were before the influence of Western contact have been inclined to emphasize a static picture of an equilibrium of associated institutions bound together by exactly repeated processes. All of this, of course, is quite legitimate and defensible, but it has caused the ethnographic literature, contrary to the perspective of archaeology, to give the impression that culture either changes explosively through some external cultural stimulus or not at all.

All of the information available on pre-contact Koriki culture indicates that it was not in static equilibrium but was experiencing distinct processes of change. To the general view, the direction of change was in many respects the opposite of what happened after contact, and the type of change was also different.

The evidence seems clear that for some time Koriki social organization experienced a radiating increase in the number of village units. Figure 1 shows the patterns of descent as old villages produced new ones. One cannot, of course, determine how long it has been since the process began, but the total pattern of village genealogy is one that is easily obtained from any adult Koriki male, and many elderly informants remembered some of the later developments.

The multiplication of villages was not associated with a deconcentration of the settlement pattern. At least for a time, each new village was located immediately next to the older ones. The result was the populous village group of Ukiaravi. In the last quarter of the nineteenth century and shortly before European contact, Ukiaravi, apparently burdened by population pressures, gave off two daughter village groups, Koropenairu and Kairu'u. The establishment of both of these new groups was in the memory of aged informants in 1954-55.

Associated changes of a similar sort were going on in the patrisib and moiety system. As each new village split from an older one it carried with it the patrilineal kinship system as a part of its internal organization. The process of division, however, produced changes in the sib-moiety structure. Figure 2 shows some of the sib changes in two lines of village development from the original village of Kairimai.

1. The multiplication of villages led to a multiplication of sibs. The transfer

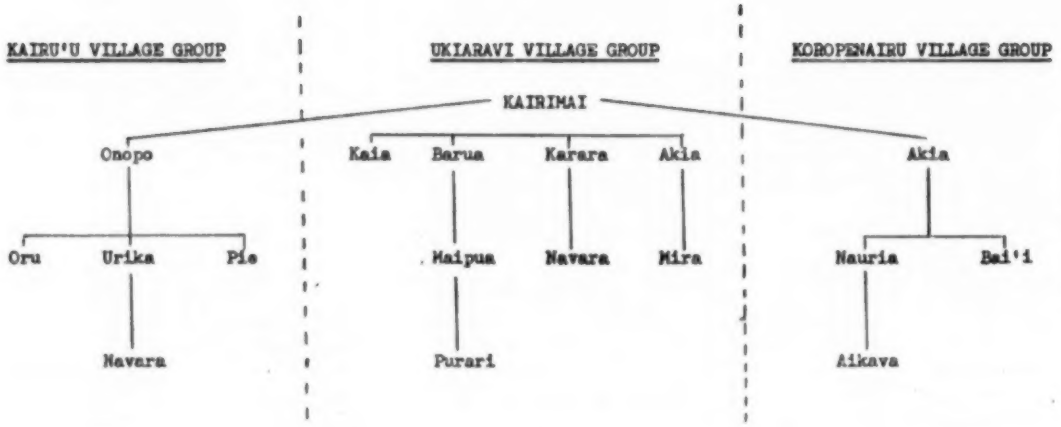

FIG. 1. Genealogy of Koriki villages at about the time of European contact.

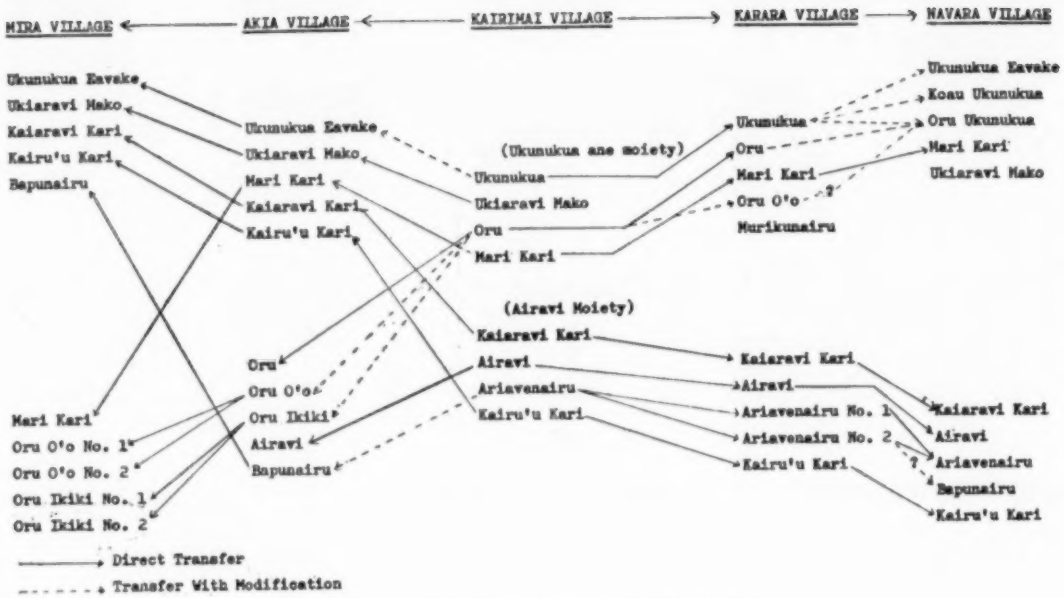

Fig. 2. Sib genealogies in two lines of descent of Koriki villages.

from old villages to new ones was not perfect, and there is a noticeable tendency to add some new sibs.

2. There was also some inclination to multiply sibs within the same village without an associated village division. Examples are Oru Ikiki nos. 1 and 2 and Ariavenairu nos. 1 and 2. Such sib division is connected with the problem of effective sib size. Each sib in a village had its own alcove in the ceremonial house. Here each man kept his sacred objects and trophies and spent much of his time. If a sib, as it was represented in a particular village, had many more than ten initiated males, it was very difficult for them all to be served by their alcove in ceremonial house. As a result some sib division occurred even without the formation of new villages.

3. Figure 2 also shows that some alteration of the moiety arrangement occurred. The sibs in both Akia and Mira villages mixed moiety indentifications to some extent, and most of them are traceable to only one of the original Kairimai moieties. One way, according to informants, that new villages could be established was for only one moiety to split away from the parent village, rearrange itself into two sides and create a new village. It would seem likely that it was such a process that created the Akia-Mira line.

4. Some sibs are absent in particular villages. In some cases the entire sib moved in a village split, leaving the older village unrepresented. From informants' testimony this seems to be the explanation of the presence of Ukiaravi Mako sib in Navara village but not in the older Karara village. But in other cases, chance fluctuations in a small population, even when it is in sum an increasing population, can depopulate small subdivisions of the larger society. In the case of a Koriki sib, total depopulation would not have been necessary. Here again, the matter revolved about some optimum number of initiated males. Just as many more than ten created pressures in the sib alcove in the ceremonial house, so much less than ten could make the sib too weak to carry out its proper role in ceremony and raiding. When the latter occurred, the remaining members of the sib joined another sib of their moiety. This relationship between sib structure, ceremonial function, and population size can be seen in Table 1. The situation depicted is in the present, but it shows that at any time a sib organization which had important functions and village limits to participation required some minimum village size above that of today. Even if a loss of functions had not dissolved the patrisib organization, contemporary depopulation would have made it an ineffective structure for its purposes.

5. Some sibs seem to have been more resistant to change in transfer from one village to another than did others. Mari Kari sib, for example, appears in all

TABLE 1

Number of Adult Males in Sibs† of Four Koriki Villages in 1955

Sibs	*Villages*			
	Karara	*Navara*	*Akia*	*Mira*
Ukunukua	2	*	*	*
Oru	12	*	0	*
Mari Kari	4	5	1	9
Oru O'o #1	5	*	2	3
Oru O'o #2	*	*	*	0
Murikunairu	3	*	*	*
Kaiaravi Kari	5	11	2	2
Airavi	6	5	8	*
Ariavenairu #1	1	1	*	*
Ariavenairu #2	5	*	*	*
Kairu'u Kari	0	3	1	0
Ukunukua Eavake	2	0	2	3
Ukiaravi Mako	*	18	6	2
Oru Ukunukua	*	0	*	*
Koau Ukunukua	*	0	*	*
Bapunairu	*	0	5	0
Oru Ikiki #1	*	*	0	7
Oru Ikiki #2	*	*	*	0
Total Adult Males	45	43	27	26
Total Village Population	157	165	79	90

† The sibs are no longer functionally present in Koriki society, but they were active recently enough for membership to be easily traced.

* Sib not present in village.

of the villages presented in Figure 2. It was the sib of the most important hereditary priest status in the Ukiaravi group, and its name literally means "priest penis." It is interesting to note how frequently the word *kari*, or penis, is present in sib names.

The changes that are evident here in the addition of new sibs, the failure of some to transfer in village divisions, and the reversal of moiety identification in some cases were obviously connected with the increase in villages and village groups, and both were the products of an expanding population. The type of change involved, however, did not lead to new types of organization but to the further development of an existing one. Regardless of the differences between Mira and Kairimai villages, the pattern was still one of concentrated settlement and a Normal Guinea type of social organization.

From what has been said, it would seem possible, with some element of speculation, to push Koriki history back to still a further stage. It seems clear that the changes that were going on in Koriki culture at the time of contact and before were predominately the responses of an existing social organization to an expanding population. Starting from that observation, the significant elements in reconstructing an earlier condition of Koriki culture can be put down in the following way.

If immigration is not a factor, an expanding population must be the result of a disbalance between the vital rates of the population in favor of the birth rate. This can be accomplished in a number of ways, but in the case of the Koriki the most obvious, for that matter the only obvious, influence in this direction was the rich and reliable productivity of the sago-collecting subsistence pattern in a swamp region with abundant sago palms. While it is possible that the Koriki lived in this productive environment for a long time without developing the technological means to exploit it, it does not seem likely. Instead it is suggested that the numerical expansition of the Koriki population was brought about by their migration into the delta swamp ecology.[15] It is not at the moment possible to know when this occurred, but it has already been noted that the population seems to have been still increasing at the moment of contact, which would indicate that sufficient time had not elapsed to allow a balance to develop between population size and subsistence resources.

Working with the assumption, then, that the increase in the Koriki population was connected with the migration of the Koriki into the sago swamp ecology, one can move a bit further with reconstructing some of the features of their pre-delta life.

1. It has been suggested that the subsistence pattern must have been substantially different to the extent that sago-collecting was not its central feature. The slash-and-burn gardening of a rather large number of domesticated plants was practiced by the Koriki for as long as Europeans have known them, but it has been distinctly subordinate to sago-collecting. In Mira, Akia, Navara, and Karara villages in 1954-55, for example, no one had a garden, although some had had one in the past and some will undoubtedly have one again in the future. The

15 Haddon argued that the Koriki, as a part of the so-called Namau social division, had migrated to the delta from an inland region in not very ancient times. See A. C. Haddon, *Migrations of Cultures in British New Guinea* (Journal, Royal Anthropological Institute, vol. 50, 1920) and *In Primitive New Guinea* by J. H. Holmes with an Introduction by A. C. Haddon (London: Seeley, Service and Co., Ltd., 1924). Holmes, in the body of his book, also gives some evidence that the Koriki's present location may not be a particularly old one. Essentially, however, this is a question to which no certain answer can be given until archaeology is more developed in New Guinea.

rather developed condition of gardening practices along with their state of relative disuse suggest that they may have been much more important in the past. It would not seem unlikely that slash-and-burn gardening rather than sago-collecting formed the core of Koriki pre-delta subsistence activities.

2. In addition to providing an abundant supply of sago, the other important aspect of the swamp ecology was the intricate network of streams it furnished for water transportation. There is virtually no place in the delta that cannot be reached by canoe. It was the combination of the sago palm and water transportation which made possible the concentrated village group settlement pattern that warfare had made desirable. We would expect, then, that the pre-delta settlement pattern was considerably more dispersed, with the people living in scattered hamlets or perhaps even separate households.

3. As has already been said, Koriki patrilocality occurs more as a result of practical problems than as a valued rule in itself. With a more dispersed population and perhaps less elaborate houses, the persistent neolocal ideal of the Koriki may have had a better opportunity to assert itself, presuming, of course, that it is an old rather than a new ideal. Murdock's analysis of social structure has stressed change in residence practices as an initiating factor in structural modification. This along with the Koriki Hawaiian cousin terminology and bilateral exogamy suggests that the patrilineal sib and moiety system may have developed after the movement into the swamp environment. Without these patrilineal elements, all other features of Koriki social organization indicate an Hawaiian type with some bilateral emphasis perhaps involving a kindred structure.

Using evidence, theory, and some speculation, the above analysis has attempted to distinguish a pre-delta culture for the Koriki and to reconstruct some of its main features. In the process another Koriki experience with culture change has appeared.

It is proposed that at some date in the past a Koriki culture characterized by slash-and-burn gardening, an Hawaiian type of social organization, a relatively dispersed settlement pattern and neolocal residence migrated into its present location in the delta swamps of the Purari River. The adaptation of the culture to the swamp ecology set in motion a process of change of a strongly reorganizational type. Sago-collecting became the subsistence base; the settlement pattern became highly concentrated; a tendency toward patrilocal residence appeared; and a patrilineal sib and moiety system was developed, changing the type of social organization to Normal Guinea.

Such was the pattern of the culture when it was first encountered by Europeans. At this time another type of change was in process. Clearly associated with the

pressures of an increasing population, this process was not basically reorganizational but tended to develop along the potential of the existing cultural pattern. The main products were new sibs, villages, and village groups.

Contact with European culture brought a new phase of reorganizational change. Initiated by the suppression of raiding activities and guided to some extent by the partial introduction of European economic objectives, Koriki culture returned to some of its pre-delta organization. The process is still under way, but many of its results are clear. The settlement pattern is decentralizing; neolocal residence is more frequent; the loss of the patrilineal kin units has returned the social organization to an Hawaiian type; and the economy has made sporadic efforts to operate in European markets. The increase in population has been reversed and considerable depopulation has occurred.

Two types of change and three phases of Koriki culture history have been distinguished here. One of the types of change is that initiated by migration into the swamp ecology at an earlier time and by contact with European culture at a later time. Its essential feature is a reorganization of cultural pattern. The second type of change was in process at the time of contact. Rather than leading to new types of organization, it acts mainly to produce further development of already existing patterns. As I understand Vogt's concepts, the first variety of change defined here is associated with what he has called "directional" processes, while the second variety is developed through "recurrent" processes. In this particular case, at least, the separation between the two types of change or processes is an analytical one. It would be as valid to view the processes going on in Koriki culture when the Europeans arrived as an integral part of the earlier adaptation to the swamp environment. A distinction, however, is useful in that it allows for the probability that some variety of change is in process in all cultural situations, while it at the same time sets apart important reorganizational change.

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SOCIAL FIELDS AND THE EVOLUTION OF SOCIETY[1]

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AMID a good deal of tumult and shouting, social and cultural evolution have taken their place in anthropology alongside biological evolution as facts of human history. There is no longer dispute that "present civilizations are historically continuous with past culture, as far back as we have evidence," that complex culture and society evolved from simpler conditions through a "process of growth, accretion, specialization, and differentiation."[2] Present debate concerns *how* the evolutionary process took place—in general, and in relation to particular sequences of change; and seeks concepts and theory that systematize or unify the vast panorama of historical time-space events which are the matrix of social evolution.

It has not been so for long. In 1939, when I discussed "Evolution in Social Anthropology" at Association meetings in Chicago,[2] I had been warned by a social scientist who was by no means extreme in his view that "evolution" was a dirty, dangerous word, and urged to replace it by the word "development." I took the position at that time that the subject matter in which evolutionary change or sequence must be studied is historical in character, and I continue to believe in this directive and this limitation. Whatever the value of a philosophy of social or cultural evolution, I think that the anthropologist, as scientist, must be true to his subject matter, its historical dimensions, and its factual limitations.

From this standpoint I want to call to mind an obvious reality of human social and cultural history that has been given too little weight in discussions of social evolution—the phenomena long recognized in such concepts as borrowing and diffusion.

As facts of history, of course, borrowing and diffusion are beyond dispute. Here I will refer only to the general evidence that they are features of human history from the beginning. Thus, in the Paleolithic, there is the distinction between the hand-ax cultural tradition and the flake tradition; each of which is of great, possibly equal, antiquity; each of which is found in many sites over a wide continental range.[3] Archaeologists agree that the distribution of each tradition implies contact and influence among many of the peoples and settlements

1 Read at the Annual Meeting of the American Anthropological Association, Minneapolis, Minn., November 19, 1960.

2 Lesser, 1952, p. 139.

3 Oakley, 1959.

who shared it. How else can we ccount for hand-axes from sites at great distances from one another so identical in form and size that often they can be distinguished only by the materials used?

Although borrowing and diffusion are evident from early times and must probably be assumed normal throughout human history, they have been taken into account for the most part only in so-called "historical" or "historical reconstruction" studies; or used primarily, in early work, as a ready (and perhaps superficial) explanation of social and cultural similarities between neighboring peoples. Little attention was given the phenomena of borrowing or diffusion in the analysis of society. That analysis was focused upon a particular social aggregate, conceived as a distinct and separate society, and the impact of other aggregates upon the one under consideration was usually treated as external, essentially fortuitous or accidental. The study of social process, social change, or social evolution became largely an analysis of phenomena found *within* a social aggregate, delimited as a social isolate. For purposes of analysis, the external social environment was stripped away; it was a realm of historical accident which obscured the system of relations that constituted the essential process of the inner life of *a* society.

But such concepts as diffusion and borrowing, and the contact to which they refer, have a fundamental implication which cannot be ignored: that any one social aggregate—group, settlement, community, tribe—is involved with others, that an interdependent influence of social aggregates upon one another is virtually universal. And there is a further implication—that this interdependent involvement of social aggregates cannot be a matter of haphazard, chance contacts. It may be possible to conceive that certain cultural items, like a story or a type of implement, may be diffused or borrowed in the course of incidental or chance contact, though even this seems historically unlikely. But it is impossible to conceive that diffusions (implied by distributions) of forms of marriage and descent, clan structure, political institutions, or systematized religious beliefs, have taken place in such a manner. Much more fundamental interpersonal relations between people of different social aggregates are required to explain such phenomena. It may be necessary to conceive human social life and human history as a time and space continuum of human association.

If that be the reality, how can it be disregarded as extraneous and adventitious in analysis of social process, social change, and social evolution? How can a single delimited social aggregate, isolated from its matrix of human associations, be treated as society or human society, and the processes of human social life be studied exclusively within it?

Yet the myth of the primitive isolate is still with us, still embedded in current concept and theory. Robert Redfield, for example, a leading spokesman for the concept of the primitive isolate, recognized in his "Peasant Society and Culture" that the conception is inapplicable to peasantry, as "part-societies," but still clung to it as the essential abstraction of primitive society and as a conceptual model which may have continuing, if more limited, usefulness in research on more complex society.[4]

Adherents of the theory of the primitive isolate, of course, recognize that it may be impossible to find and difficult to conceive *a* society that is physically or socially isolated in an absolute sense at any level from primitive to modern. Yet they imply that isolation, as the extreme opposite of the contact so obvious in complex social situations, be imagined to have been the condition at the beginning; and draw from this hypothetical notion the idea of the primitive isolate as a conceptual tool. One is reminded of how some 19th century evolutionists postulated promiscuity—the absence of patterned interpersonal sexual relations—as the primitive social condition from which marriage and the family had evolved.

The conception of the primitive isolate compels one to view early or primitive human life as a world of closed social aggregates, each out of contact with other humans.[5] It involves what Gideon Sjoberg has referred to as a preoccupation with "closed systems,"[6] of which both anthropologists and sociologists have been guilty; and it would appear that much of the discussion of "holism" as *the* anthropological approach entails this preoccupation. The early functionalists who made a fetish of exclusively synchronic study left diachronic phenomena, history, out. History in their view might be interesting in a descriptive sense; but, with its concern for borrowing and diffusion, it was about "origins" and irrelevant to the analysis of social process within a closed social system.

I propose to ask what difference it makes if we adopt as a working hypothesis the universality of human contact and influence—as a fundamental feature of the socio-historical process; if we conceive of human societies—prehistoric, primitive, or modern—not as closed systems, but as open systems; if we think of any social aggregate not as isolated, separated by some kind of wall, from others, but as inextricably involved with other aggregates, near and far, in weblike, netlike connections.

The concept of social field, or field of social relations, being developed and

4 Redfield, 1956, chapters 1, 2. Also see Redfield 1947, 1953, 1955.

5 A general view implicit in Sumner's (1906) influential "Folkways." Wirth, 1938, remarks that Sumner pictures a primitive society "as small isolated groups of human beings scattered over a vast territory."

6 Sjoberg, 1959, pp. 352-353.

used by British social anthropologists—Firth, Fortes, Gluckman, Barnes,[7] among others—seems to fit the realities of socio-historical human situations, so described. Firth has put it concisely: "Fields of social relations, not clear-cut societies, must be the more empirical notion of social aggregates."[8] In this discussion I suggest the concept be understood in the sense of Durkheim's statement of many years ago: "There is no people and no state which is not part of another society, more or less unlimited, which embraces all the peoples and all the states with which the first comes in contact, either directly or indirectly."[9]

The concept of social field has a fundamental implication: social relations within the field are patterned, not unstructured, adventitious, or incidental. The concept of patterns of culture is now a byword. But it has been used almost exclusively to describe interpersonal relations between members of the same social aggregate. Yet the evidence strongly indicates that interpersonal relations between members of different social aggregates—groups, tribes, communities, settlements—are similarly patterned. One calls to mind Malinowski's famous kula ring of the Trobriands. Yet that is not an exceptional phenomenon. Where trade relations among primitives are explicitly described—American Indian, Australian, Melanesian, African—trade partnerships and/or structured friendships—traditional patterning of interpersonal relations—are similarly in evidence. Intermarriage, visiting and travel, feud and rights of asylum, war and peace-making and attendant capture and/or adoption involve in the same way patterned intergroup relations that can be described in terms of norms and traditional behavior. G. C. Wheeler pointed this out long ago in his "The Tribe and Intertribal Relations in Australia."[10]

Much of the phenomena of borrowing and diffusion, as already noted, imply structured interpersonal relations. For example, Goldman showed some years ago how the incongruous diffusion of potlatching and inherited crest prerogatives from the affluent Northwest Coast to the interior Alkatcho Carrier of British Columbia, with their relatively impoverished economy, could be understood in terms of trade and marriage between the Carrier and the Bella Coola. Long established trade relations, intensified by the coming of the fur trade, stimulated intermarriage between coast and interior families to consolidate and maintain trade connections. Marriage brought with it, as an integral part of its pattern, potlatch feasting and

7 Firth, 1951; Fortes, 1949; Gluckman, 1949; Barnes, 1954.

8 Firth, *op. cit.*, p. 28.

9 Durkheim, 1915, p. 426: quoted in Honigmann, 1959, p. 17. Honigmann distinguishes "community" and "society," using a field concept for the latter.

10 Wheeler, 1910.

inherited prerogatives.[11] Again, in the Southwest, the diffusion of clan forms from Pueblo peoples to Athapascan Navahos and Apaches involved long continued association—including trade relations between particular Pueblos and particular Athapascan groups during the early period of Athapascan penetration of the region—and residence and marriage, at various times, of Pueblo people among Navahos and Apaches.

Thus, within a field of social relations interpersonal relations are structured and patterned. Conversely, when structured or patterned relations are found—or are implied by other evidence—a field of social relations is involved, embracing more than one social aggregate and usually a considerable number.

The social field concept, so understood, can I believe help to resolve important problems of social evolution. In this brief discussion I propose only to offer some illustrative suggestions—in particular, on the appearance of surplus and the origins of the division of labor.

The traditional view of surplus in discussions of social evolution, followed by Childe and others, is essentially that it is an unanticipated emergent of technological development—the domestication of plants, initially. Technology is in this view an independent variable, its development or evolution left unexplained. Technological changes lead to increased food production, in quantities greater than those needed by the producer, resulting in surplus. Surplus in turn is then a precondition of and a factor in the appearance of craft specialization. Some members of the group produce food for all; some are released for other work. Under some form of leadership or group authority, a division of work may then even be assigned.[12]

This view betrays the preoccupation with social aggregates as closed systems referred to above. If social aggregates are viewed as open systems in a field of social relations, it seems clear that Neolithic cultivation of plants developed in a region in which widespread intergroup relations were going on, including economic relations of exchange as well as social relations. There is I believe adequate evidence, summarized by Childe himself, for Neolithic and even pre-Neolithic trade. In addition, I think it can be stated as a general hypothesis that in *any* region in which social aggregates differ in what they produce, exchange between aggregates is probable. In the Neolithic, in maintaining or extending social and economic relations, those at either end of the exchange found it advantageous to produce what they habitually exchanged or could exchange in sufficient quantities to meet their exchange obligations as well as their own needs. Demand then, in the sense of

11 Goldman, 1941.
12 Childe, 1936, 1946, 1951.

obligations and requirements involved in on-going exchange relationships, was related to supply. *We* refer to what was produced for exchange, and not for immediate domestic needs, as *surplus*.

This conception clarifies the meaning and appearance of surplus and at the same time indicates the motivations probably involved. It is suggestive too on the problem of technological improvement and invention, and why it may happen, making such change an interdependent rather than an independent variable. Production *may* be increased to meet obligation demands without change of technology—by planting more fields in the same manner, for example. *Or,* it may be increased by inventing or discovering new, more productive techniques. The appearance of hybridization during the Neolithic, for example, obviously implies intention and purpose. It can hardly have been an accidental discovery or some inevitable technological evolution independent of the social situation in which it occurred and the human motivations active therein.

Thus, from the social field approach, technological change and improvement as well as the appearance and meaning of surplus are functional aspects of a field of social and economic relations.

The appearance of division of labor can be similarly understood. Differences in the products exchanged between members of different social aggregates may result in group specialization. Members of one group become more dependent on members of another for certain products secured by exchange, and as a result may become regional specialists in the production of the goods required for exchange. In the American Indian plains, for example, during the pedestrian period, Apaches and Kiowas traded meat and skin products with Arikara villagers for tobacco and corn.[13] Although within each Plains group activities were differentiated only by sex and age—each man a hunter, each woman a skin worker—the men smoked tobacco and men and women ate corn which they did not produce, which they obtained by trade. Group specialization and a regional division of labor were taking place within a field of economic relations.

Childe refers to this type of situation, in discussing prehistory, as *inter*communal specialization, and distinguishes it from *intra*communal specialization.[14] The data he provides suggest that the former—intercommunal specialization—may precede the latter—intracommunal specialization—in time and in evolution. But *intra*communal specialization, division of labor within one social aggregate, seems primarily a more intensive development of group specialization in relation to regional exchange, and its appearance *can* be contemporary with intercommunal

13 Hyde, 1959.

14 Childe, 1951. See index for page references to specialization.

specialization, rather than later. Thus, when members of a single social aggregate produce more than one product which is involved in regional exchange and group specialization, specialization may arise within the group as different individuals concentrate on different products to meet their exchange obligations. This may be seen in as primitive a society as the Bushman of South Africa. Bands that are linked in a chain of trade for iron and tobacco show tendencies toward both group specialization and individual specialization. The latter occurs where individuals may completely specialize as arrow-makers or even honey-gatherers and trade not only outside their own bands but within them for other products they need.[15]

Conceiving the origins of division of labor in this way as a functional aspect of a field of economic exchange relations avoids the artificial over-rationalization of the traditional view. The idea that division of labor follows from surplus seems to call for a deus ex machina—some central agency which in effect plans and institutes a division of labor and assigns work activities. A field theory makes the interpolation of such extraneous factors unnecessary. At the same time it overcomes difficulties inherent in the vew that the beginnings of division of labor can be found in the social and economic structure of particular social aggregates by studies of part-time specialization—like that of the tipi pattern-cutter or the tipi-painter of the American Indian plains. Full-time specialization is not accounted for in such limited studies; the factors and functional relations included are not adequate by themselves to explain it. The theory proposed, by contrast, provides an adequate basis of explanation by enlarging the analysis to take into account factors of exchange or trade, of economic and social relations within a larger field.

The hypothesis that division of labor arises as a function of a field of economic relations and exchange offers a general theory that can be analyzed and tested in many historical situations. For example, it supports the thesis that pastoral cultures, even in early times, were probably specializations or part-cultures in the economic sense, linked in a symbiotic relation to agricultural peoples in a social and economic field.[16] Or, in the modern period, it seems clear that the industrial revolution in England, essentially a further intensification of division of labor, arose while England was part of a far-flung web or network of mercantile trade relations. The rapid growth of the triangular trade with coastal Africa for slaves and with the West Indies for plantation products was a major factor in stimulating production of manufactured goods at home; first by causing an increase in domestic handicraft production, later by stimulating the invention of new techniques for further increas-

15 Schapera, 1930, pp. 145-147.
16 Kroeber, 1948, pp. 276-277, 690-691.

ing production. Trade demands made for increased production and then improved methods of production.

The social field concept as the basic social reality, rather than distinct societies or social aggregates, may clarify other evolutionary problems, for example, the evolution of political and legal institutions. It suggests that centralized political or legal authority develops within a field of political and legal relations, and not in isolated, closed social units considered separately. The conquest theory of the state, stripped of its particularities, essentially means that the *relations* of social aggregates within a field are the primary factors in the development of state forms and class-structured states.

The suggestion offered, then, is that the field concept rather than the concept of society as an isolate or closed system reflects socio-historical realities more accurately—that human contact and patterned interpersonal relations are not restricted to social interaction within a single localized social aggregate, but are a universal constant of social life. From this standpoint, there is a communications process between any social aggregate, primitive or more advanced, and others. The communications process has evolved, but it has not evolved out of conditions in which it was absent, but from the primitive communications process which links social aggregates even in the earliest time of which we have evidence. [17]

The field concept, I suggest, is particularly useful in understanding social evolutionary change that has taken place in human history. It unites into one field the study of those patterned interpersonal relations usually considered external, or merely a matter of historical accident, and those that are an integral part of a particular social aggregate. It breaks down the notion that history involves mere happenstance which interferes with analysis of social process in systems of relations, of order and regularity in events. History and synchronic analysis become parts of one universe of discourse, of one order or level of the human social process.

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STRUCTURAL CONTRASTS BETWEEN SYMMETRIC AND ASYMMETRIC MARRIAGE SYSTEMS: A FALLACY[1]

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DURING the past twelve years considerable advances have been made in the structural analysis of prescriptive marriage systems. Lévi-Strauss and his students in France, Leach and Needham in England, J. P. B. de Josselin de Jong, P. E. de Josselin de Jong, Fischer, Held, van Wouden and others in the Netherlands, Homans and Schneider and Livingstone in this country are among those who have noted that prescriptive marriage systems become more comprehensible when marriage is viewed as an exchange between various kinds of social groups.

The view seems to be universally held that the sorts of exchange which take place may be categorized as of two major kinds: first, direct exchange, in which men of group A give women in marriage to men of group B and men of group B reciprocate by returning women in marriage to men of group A. This type includes a number of Australian systems involving "sister-exchange" or "symmetrical" or "bilateral" cross-cousin marriage. Examples would be so-called four-section systems in which male ego marries his mother's brother's daughter who is structurally also father's sister's daughter, and eight-section systems in which male ego marries his mother's mother's brother's daughter's daughter who is also father's father's sister's son's daughter.

The second type is indirect exchange in which men of group A give women in marriage to men of group B but men of group B never reciprocate in kind. Instead, men of group B give their women in marriage to men of group C, the movement of women being unidirectional. This sort of arrangement is associated with prescriptive matrilateral cross-cousin marriage in which male ego marries mother's brother's daughter who is not at the same time father's sister's daughter. That is to say, sister-exchange is not permitted and the two kinds of first cross-cousins are thereby kept structurally distinct. The minimum number of groups required for such an exchange system is three—ego's own group (containing his sisters); a second group containing his father's sister's daughters whom he may not marry; and a third group including his mother's brother's daughters or potential wives. If these three groups are A, B, C, then men of A give women to B, men of B give women to C, and men of C give women to A. The indirect exchange is implied

1 This paper was read at the fifty-ninth annual meeting of the American Anthropological Association at Minneapolis in November 1960. The research was conducted during tenure of an Andrew Mellon Postdoctoral Fellowship at the University of Pittsburgh.

in the notion that the women group C gives to group A are indirect compensation for the women that A gave to B (see Figure 1).

Systems of this type are found widely distributed in Indonesia and occur in other ethnographic areas as well. It has been argued that because direct reciprocity in kind is never practiced, every group standing in the relation of wife-giver to some second group and wife-receiver to a third group, that this sort of system implies status inequalities, as between the groups involved. This has led to a number of speculations regarding the extra-kinship implications of this structural type and an insistence that matrilateral cross-cousin marriage represents a special case and that, consequently, systems involving this marriage prescription differ in kind from systems involving direct reciprocal exchanges of women.

I venture to suggest that while status inequalities may sometimes fit with a system of matrilateral cross-cousin marriage, as appears to be the case in much of Indonesia, they are no necessary counterpart and in certain circumstances may be quite impossible. This is because while the over-all system may be viewed as asymmetric in an abstract structural sense, the actual exchanges of women which take place among local or kin groups may follow a rigid rule of direct reciprocity.

This problem came to my attention in the course of field work in northern Pentecost in the New Hebrides. There marriage is asymmetrically prescribed with a second cross-cousin, but the natives view the situation as one of direct reciprocal exchange. Exposition of the northern Pentecost data would introduce complexities beyond the scope of this paper and a full description of the system will be published elsewhere. For present purposes I propose to illustrate the point with hypothetical cases.

In Figure 2 the circles A, B, C, D represent local patrilineages. Every man marries his mother's brother's daughter who is not father's sister's daughter. Viewed from the usual point of view, the movement of women is unidirectional and there is never any direct reciprocity. But this picture only holds if we operate with the notion that men must exchange women of their own lineages. Viewed from another perspective it can be seen that direct reciprocal exchanges of women obtain between men of groups A and C, B and D. From this point of view, men of A give their nieces (sisters' daughters) to men of C and men of C give their nieces in return to men of A. The men of B and D likewise exchange nieces.

It may be objected that in societies possessing patrilineages, one would expect jural authority over women of a lineage to be vested in male members of that lineage (that is, fathers or brothers) rather than in the mother's brother who belongs to a different lineage. This is a common assumption of anthropologists, but the point should be settled by ethnographic inquiry. Unfortunately, much of the

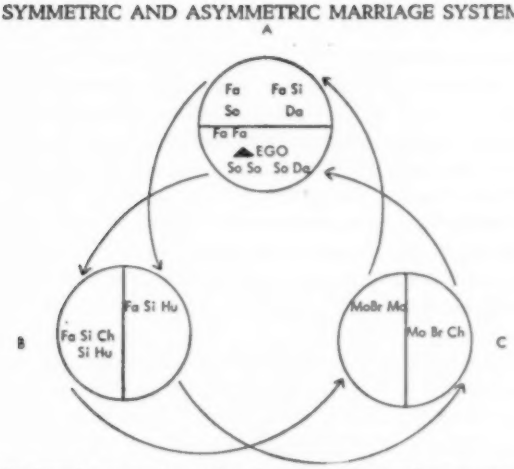

FIG. 1. Matrilateral cross-cousin marriage showing unidirectional movement of women among patrilineal groups represented by circles A, B, C. Arrows represent direction women move at marriage. In this and subsequent diagrams each circle is divided in half in order to separate adjacent generations. This is to enhance clarity in illustration and does not imply a formal generation distinction in the society.

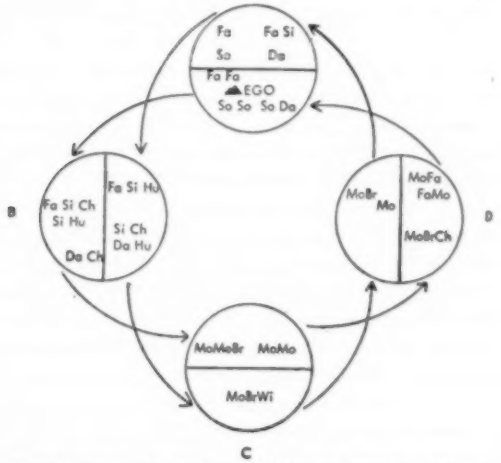

FIG. 2. Matrilateral cross-cousin marriage resulting in "niece exchange" or "granddaughter exchange." Circles represent patrilineal groups.

relevant literature is silent on this point. In any event, it need not trouble us here, for reciprocal exchanges of women may be explicitly recognized and expressed without reference to jural authority. In the situation diagrammed in Figure 2, it does not matter whether male ego says he gives his sister in marriage to father's sister's son, or alternatively, whether he says he gives his daughter in marriage to sister's son, or whether some other transfer is conceptualized. Regardless of the locus of jural authority, the participants may view the system as one of direct exchange by observing that "we men of A marry the nieces (sisters' daughters) of men of C and they marry our sisters' daughters." Or alternatively, "we men of A marry the daughters' daughters of men of C and they marry our daughters' daughters."

It should be noted that the men (and women) of A are subdivided into two groups, as are the members of B, C, and D, and that exchanges actually take place between men of paired subdivisions. Thus if ego conceives as his exchange partners those men in C who are actual or classificatory mother's mother's brother's sons and who fall into the same subdivision as mother's brother's wife, then this would be categorized as "niece exchange." Alternatively, if ego conceives as his exchange partners those men in C who belong to the subdivision of mother's mother's brother, then "granddaughter exchange" results. In either case, *all* the men of A exchange nieces or granddaughters (depending upon which alternative is perceived) with *all* the men of C, even though exchanges actually take place between men of paired subdivisions.

It is clear from the foregoing that while in structural terms, a system based on matrilateral cross-cousin marriage can be viewed as asymmetric, it is no necessary corollary that actual exchanges of women between local groups are so defined or regarded by the participants. On the contrary, it is quite possible for the exchange of women to be viewed as directly reciprocal. It follows that the statement of a marriage rule in asymmetric terms, for example that a man must marry his mother's brother's daughter and may not marry father's sister's daughter, of itself does not indicate whether local groups will exchange women on an asymmetric or a symmetric basis. If they are to exchange on a symmetric basis, certain conditions must obtain. First, there must be a minimum of four groups. Beyond this, the number of groups need not increase by pairs. Assuming some variation in the size of local groups, any number beyond four might support such a system. It follows from this that neither a statement of the matrilateral cross-cousin prescription, nor a statement of the number of intermarrying groups in the society (unless the number be less than four, an unlikely event), nor yet the two together, is sufficient to establish the existence of asymmetric exchanges of women among

local groups. Further, if in a given society the exchange of women is viewed by the participants as directly reciprocal, it is difficult to see how status inequalities associated with wife-giving or wife-receiving could arise.

We turn now to consideration of another asymmetric marriage prescription—that of patrilateral cross-cousin marriage. This is less frequently reported and requires that male ego marry father's sister's daughter, but may not marry mother's brother's daughter. Reciprocity of the sister-exchange type is thus precluded, a minimum of three groups again being required as with matrilateral cross-cousin marriage. At the same time, patrilateral cross-cousin marriage is not the mirror image of matrilateral cross-cousin marriage. Despite the fact that a minimum of three groups is required—ego's own group including his sisters, a second group including mother's brother's daughters whom he may not marry, and a third group including father's sister's daughters or potential wives—it has been pointed out that patrilateral cross-cousin marriage nevertheless results in an exchange of women between two groups, the exchange being completed after a time lag of one generation. In a patrilineal system if a woman of A marries a man of B, their daughter eventually marries a man of A and in this sense may be regarded as a return for the woman A originally sent to B. In a matrilineal system the exchange is not precisely the same because a daughter always belongs to her mother's group. Nevertheless, the structural picture remains unchanged because A stands in the relation of wife-giver to B in one generation and wife-receiver to B in the next. With the exception of Needham,[2] who has recently suggested that a system based on prescriptive patrilateral cross-cousin marriage cannot exist, I believe the above interpretation of exchange relationships entailed by this prescription is generally held.

So far as I know Needham is the only one who has challenged the notion of prescriptive patrilateral cross-cousin marriage, although others such as Eggan have noted the lack of adequate ethnographic description. Needham's argument has been challenged from several quarters and he has commented in print recently as follows: ". . . though I remain sure that the conclusion is correct I now think I made a mess of expounding the argument for it. An expanded and amended version will appear elsewhere."[3] Pending further explication of the issues, we may consider prescriptive patrilateral cross-cousin marriage as a hypothetical case. Needham has effectively demonstrated the lack of adequate descriptions of this type.

The point I wish to make regarding patrilateral cross-cousin marriage is that it can be viewed as a system of direct reciprocal exchanges in much the same way

2 R. Needham, *The Formal Analysis of Prescriptive Patrilateral Cross-Cousin Marriage* (Southwestern Journal of Anthropology, vol. 14, pp. 199-219, 1958).

3 R. Needham, *A Structural Analysis of Aimol Society* (Bijdragen tot de Taal- Land- en Volkenkunde, vol. 116, pp. 81-108, 1960), p. 82.

as suggested for matrilateral cross-cousin marriage. Figure 3 illustrates this point. The circles A, B, C, D represent four matrilineages. Every male ego marries a

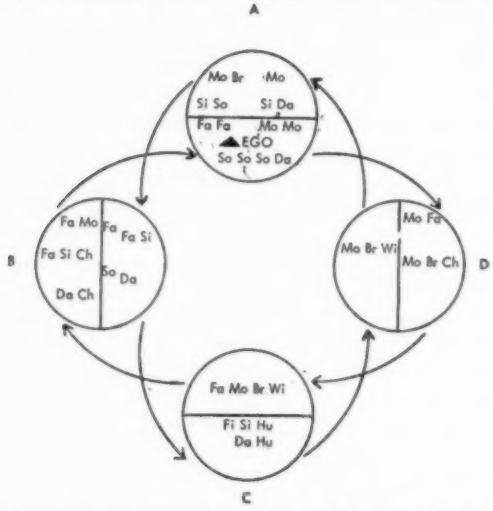

FIG. 3. Patrilateral cross-cousin marriage resulting in "daughter exchange." Circles represent matrilineal groups.

father's sister's daughter who is not mother's brother's daughter. The conventional view states that A stands in relation of wife-giver to B in one generation and acts as wife-receiver from B in the next. Similarly, A is in alternate generations wife-receiver and wife-giver vis-à-vis D.

From the point of view proposed here, direct reciprocal exchanges occur *every generation* between men of A and C, men of B and D. Men of A give their daughters to men of C and men of C give their daughters in return to men of A. Similarly, men of B and D exchange daughters. We may note that if men move into avunculocal residence at or before marriage, and if women reside with their parents until marriage when they go to join their husbands, then the direct exchange between lineage groups suggested here is a concrete reality in terms of local groups. The men in each of the circles may be conceived as living in avunculocal residence. The women should be located with their spouses if married, the unmarried women with their fathers. Ego's daughter is born at A and on marriage goes directly to C. Similarly mother's brother's daughter is born at A and on mar-

riage moves to C. Women born at B, sister for example, at marriage move to D and women born at D move to B. Direct reciprocal exchanges of women can thus be very evident at a local level.

One further point remains. I have tried to show that with prescriptive asymmetric cross-cousin marriage, direct and reciprocal exchanges of women can occur. I have displayed this using matrilateral cross-cousin marriage in association with patrilineages and patrilateral cross-cousin marriage with matrilineages. Ethnographic reports indicate a high correlation of the matrilateral preference in patrilineal societies whereas the patrilateral preference is usually recorded in societies following matrilineal descent. Various hypotheses have been brought forward to account for these correlations, usually involving recourse to factors of affect or jural relations—factors which the structuralists normally prefer to exclude. Some of the latter, Lévi-Strauss and Leach for example,[4] have gone so far as to deny that the type of lineality has any significance in the study of asymmetrical cross-cousin marriage.

In this connection I should like to point out that the direct reciprocal exchanges of women displayed in Figures 2 and 3 do not occur if we try to reverse the lineality of descent. Matrilateral cross-cousin marriage in association with matrilineages, or patrilateral cross-cousin marriage with patrilineages does not appear to produce symmetric exchanges of women. I submit, therefore, that if systems involving these prescriptive rules are in any way concerned with maintaining reciprocal exchanges of women, then the high correlations ethnographically recorded may reflect purely structural limitations on these systems.

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4 C. Lévi-Strauss, *Les structures élémentaires de la parenté* (Presses Universitaires de France: Paris); E. R. Leach, *The Structural Implications of Matrilateral Cross-Cousin Marriage* (Journal, Royal Anthropological Institute, vol. 81, pp. 23-55, 1951).

NEOLITHIC CULTURES OF THE SUNGARI VALLEY, MANCHURIA

KWANG-CHIH CHANG

IN THE HEART of Manchuria lie the erosion plains of the Sungari valley, forming a vast low but hilly country delineated in the north by the Khingan Mountains and in the southeast by the Long White Mountains. To the south, the plains continue on through the Liaoho drainage of southern Manchuria and merge with the alluvial plains of North China in the Bohai Bay area. The Sungari River, a tributary of the Amur, 1,130 miles long, runs from southern Manchuria northeasterly, joined by the Nonni River (Nen Kiang) coming from the north, and by the Mutankiang River from the southeast. It enters the Amur lowland along a valley where hills close in on either side, in the eastern extreme of Manchuria (map, Fig. 1).

The first investigations of Neolithic sites in the Sungari valley date back to 1927 when Torri Ryuzo, a Japanese anthropologist, found several prehistoric localities in the middle Sungari valley.[1] During subsequent years, a series of significant sites were found at Anganghsi, near Tsitsihar;[2] along the shores of the Lake Kingpai in the Mutankiang valley;[3] and in the suburbs of the city of Kirin.[4] These investigations have revealed a phase of Neolithic culture, distinctive of the area but bearing striking resemblances to Neolithic cultures of North China and the Liaoho drainage on the one hand, and to the Stone Age remains in the Maritime Province of the U.S.S.R. on the other. During the last decade, extensive surveys have been conducted in the Nonni valley,[5] the upper Sungari around the cities of Kirin and Ch'ang-ch'un,[6] the vicinity of Harbin,[7] and the middle and lower Mutankiang River.[8] These archaeological surveys have not only served to confirm the results obtained prior to the War but greatly amplified our knowledge concerning Neolithic cultures in this area.

The location of the Sungari is highly significant: it is situated at the meeting place of no less than four ancient centers of Neolithic cultures, i.e., the Huangho valley, the desert and steppe zone of Mongolia, the Pacific coast, and the taiga zone around Lake Baikal. Although encircled by mountains on three sides, the fertile Black Soil of the Sungari basin was accessible, to a greater or lesser extent, from all of the four areas in question. The Neolithic prehistory of the Sungari

1 Torri, 1933.
2 Loukashkin, 1931a, b; Teilhard de Chardin, 1931; Liang, 1932.
3 Ponosov, 1938.
4 Mikami, 1939; Li, 1946.
5 Lü, 1960.
6 Tung, 1955, 1957; Kang, 1955, 1957; Wang, 1960a.
7 Chao, 1960b.
8 Li, 1954; Chao, 1960a; Tan and Sun, 1960.

valley, to be sure, is still far from satisfactorily understood, but a preliminary survey of the literature is called for and may prove of interest to prehistorians of North Asia at large.

THE NONNI VALLEY SITES

In the Nonni valley, as well as in eastern Mongolia, directly overlying the riverine-lacustrine sandy deposits of the loessic stage (Fourth Glacial) was a Black

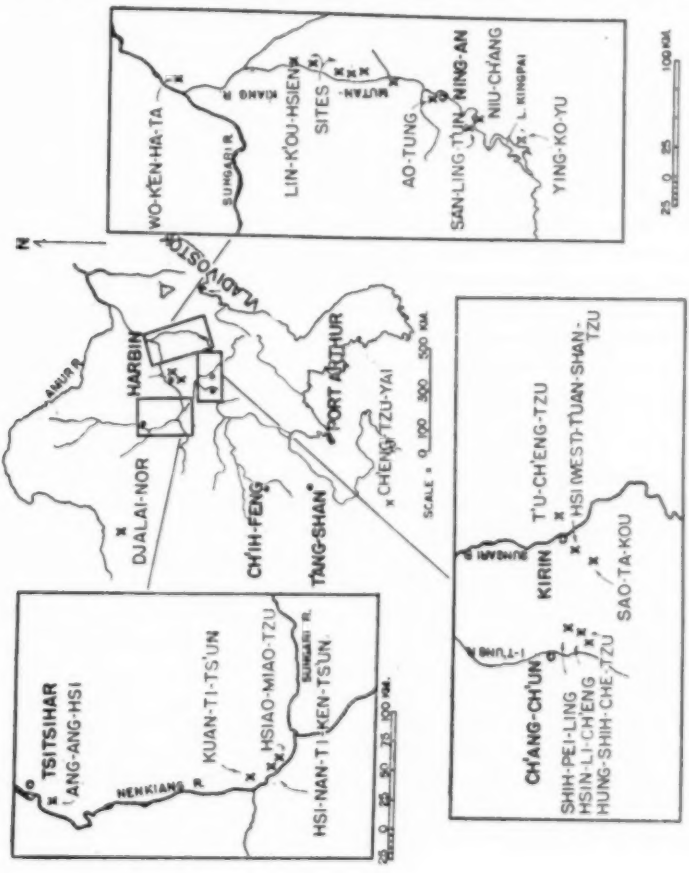

Fig. 1. Map of Manchuria showing the location of the Sungarian Neolithic sites.

Earth stratum, probably remains of an ancient forest cover formed during the earlier post-Pleistocene periods. It was gradually transformed into a yellowish and sandy layer, the deposition of which began when desiccation set in. Eolian and erosional actions of later post-Pleistocene have cut this ancient land surface up into a series of sand-dunes along the river valleys in some places and river terraces in others. Neolithic remains have been collected from the Black Earth stratum and from the lower portions of the modern sandy deposits at several localities in the lower Nonni valley, south from Tsitsihar. The most important one is Anganghsi, discovered by A. S. Loukashkin in 1928 and excavated by Liang Ssu-yung in 1930. In October, 1957, the Heilungkiang Province Museum conducted an extensive survey along the left bank of the lower Nonni south from T'ai-lai Hsien, locating no less than eighteen Neolithic sites, among which three are significant: Kuan-ti-ts'un, Hsi-nan-ti-k'en-ts'un, and Hsiao-mioa-tzu.[9]

Anganghsi[10]

At the site of Anganghsi, about six kilometers from the Anganghsi railroad station to the east and seven or eight kilometers away from the Nonni River to the west, in an area approximately 1,100 meters in length, Liang investigated four sand-dunes. On the exposed surface of the Black Earth stratum, Liang and Loukashkin collected about a thousand pieces of stone implements and potsherds, and two burials were found in one of the sand-dunes.

One of the burials was that of an adult male, heading north and lying on his back. No grave pit was located, and the post-cranial bones had been badly dislocated, facts that led Liang to speculate that the dead was simply laid on the ground, without being inhumed, together with accompanying goods. These consisted of a round-bodied, flat-bottomed pot, placed about fifteen centimeters to the left of the skull; six small flakes mixed with the decomposed ribs; and one polished stone adze, twelve bone artifacts (six harpoons and points, three awls, a bone handle grooved for side-blade insertions, and three others whose use was not clear), a polished horn, fragments of a spouted pot, bird bones, and a sawn femur of deer, placed at the distal end of the skeleton. South of this burial was a second which was badly disturbed and consisted of a pile of fragmentary human bones, many stone implements, some bone tools, a round pot, and a spouted vessel.

The stone implements from the Anganghsi site, almost exclusively collected from the surface, were made primarily from flint and chalcedony; agate, silicified slate, jade-like rock, basalt, marble, and quartz were also utilized to a small extent.

9 Lü, 1960.

10 For brief summaries in Western languages, see Cheng, 1959, pp. 137-141; Teilhard de Chardin and Pei, 1944, p. 32.

Typologically, according to Liang,[11] they fell into two classes: (1) microliths, and (2) non-microliths. The first class consisted of micro-cores (some utilized), microblades, retouched trapezes, points and arrowheads, end-scrapers, and so forth. The second class, on the other hand, included flaked triangular and rectangular knives (or scrapers) and pressure-flaked arrowheads (straight- or concave-based) and long, leaf-shaped points, polished adzes, and six perforated beads. Among the bone implements, the barbed toggle harpoon heads, barbed spear-heads and fish-spears, a grooved handle, and two fragments that might have been isolated parts of bone plate armor, are particularly noteworthy. Pottery from this site was all hand-made, often built in several sections. Dark and light brown were the dominant colors, and the paste was often sand- or shell-tempered. The rim sherds showed straight and simple walls, and the bottom of the pot was exclusively flat. Occasionally a spout was attached. The decorations were in a band confined to the upper part of the body. They were incised with a pointed instrument or with fingernails, appliquéd, or impressed with cord-marks. The design was entirely rectilinear and geometric: parallel lines, cross-hatches, triangles, and rows of punctate marks. The size of vessels is uncertain; the two complete vessels both measured a little over ten centimeters in height. Animal bones excavated from the Black Earth stratum included those of frogs, fish, birds, pigs, deer, hares, and dogs.

The burials and artifacts at the site of Anganghsi indicate a prehistoric culture of lacustrine hunter-fishers, possibly with domesticated dog, but yielding no evidence of agriculture.

Kuan-ti-ts'un

A Neolithic site was situated atop a small hill, between the Nonni and the village of Kuan-ti, in the territory of the Autonomous County of the Durbot Mongols. It was a dwelling site with thin (10-15 centimeters) cultural debris. Pottery, stone implements, and bone artifacts were collected. The potsherds were reddish, light yellow, and greyish-black in color, some sand-tempered. Decoration consisted of engraved and incised geometric designs (triangles and lozenges, some of which bore close similarities to the incised and engraved designs on the pottery of Hsiao-t'un and Chu-chia-chai in north China), appliqués, and fingernail marks. A few whole vessels indicated the presence of small flat-bottomed cups (a little over ten cm in height), and a fair number of *li*-tripods. The *li*-tripods were hollow-footed and beautifully engraved or solid-footed and with a plain and coarse surface. There were also several small and hemispheric clay objects (about 5 cm high), the use of which is not clear. Stone implements were few; they consisted of a core, two arrowheads, and a fragmentary chisel of slate. Bone artifacts

11 Liang, 1932, pp. 11-30.

included six perforated plates (of armor ?), an awl, and two incised bird bones which resembled the needle-case of northern Asia. Animal bones were those of fish, birds, small mammals, wolves, and deer, and there were fish-scales and mollusk shells.

Hsi-nan-ti-ken-ts'un

On several terraces and sand-dunes south of Hsi-nan-ti-ken village were scattered many prehistoric remains, among which were potsherds and stone artifacts. The sherds were black and yellow; some of the light yellow ones were painted in red pigment. The surface was plain or burnished. There were also a few red sherds, slipped in red. The pottery was in the form of *li*-tripods, pots, bowls, and basins, and the decoration was similar to the Kuan-ti-ts'un ware, except for the addition of painted wares and the scarcity of fingernail prints here. Stone tools were few; they were of the microlithic category and of flint and agate. Fish bones and fish scales were found in great quantity, in addition to some bird bones and mollusk shells.

Hsiao-miao-tzu

Neolithic sites were found on small hills northwest of the town of Hsiao-miao-tzu. There pocket-shaped storage pits were excavated, in which potsherds, animal bones, fish bones, fish scales, and mollusk shells were found. Here the pottery seemed to be of larger size and had been more crudely made than that of the two preceding sites. Wares were sand-tempered, black and greyish in color, and without decoration. *Li*-tripods were absent but pots, bowls, cups, and basins were found. It was noted by the investigators that the ceramic ware of this site differed considerably from that of Kuan-ti-ts'un and Hsi-nan-ti-ken-ts'un in the matters of paste, firing, color, and technique of manufacture, but resembled the pottery of Anganghsi. Microliths were collected here from the surface; they were of agate and flint. On the surface were also fish bones, fish scales, mollusk shells, and bones of small mammals.[12]

It seems reasonable to group these four prehistoric sites in two classes: (1) Anganghsi and Hsiao-miao-tzu, and (2) Kuanti-ts'un and Hsi-nan-ti-ken-ts'un. Both of these may belong to the same culture tradition, characterized by the manufacture of microliths, armor of bone plates, and brown and greyish sand-tempered and flat-bottomed pottery with incised geometric decorative designs. On the other hand, the first group is distinctive for its abundance of microliths and the absence of *li*-tripods, whereas in the second group there is relative rarity of microliths but the presence of *li*-tripods, red and yellowish, burnished sherds, painted sherds, and sherds with engraved geometric designs. It appears probable that the

12 Lü, 1960.

first group represents an earlier stage of culture and the second a later stage during which influences from the Huangho Neolithic were received. During both of these stages, the inhabitants engaged in fishing, riverine hunting, and collecting, as indicated by their cultural inventories and the faunal remains, but the decrease of microliths during the second stage may suggest a decline of hunting activities. Whether this resulted from the growing importance of fishing or was due to the introduction of agriculture, there is still not enough evidence for conclusion.

THE UPPER SUNGARI VALLEY: KIRIN AREA

Neolithic sites in the Kirin area were first discovered in the late 'thirties during the later part of the Japanese occupation of Manchuria,[13] but only when the Communist archaeologists of the Kirin Museum began extensive surveys have remains of Neolithic settlements been uncovered widely and our knowledge concerning the prehistoric life and stratigraphy very considerably enhanced. A synthetic report issued in 1957[14] listed twenty-eight sites. These were distributed in the vicinity of the city of Kirin, on the hilly terraces, averaging fifty meters above the surrounding level, along both banks of the Sungari River. There were sites on the low plains of the Sungari but they apparently belong to a slightly later period of time. These sites were ancient settlements of varying size (ranging from a few thousand to several dozen thousand square meters) and of relatively short duration. The cultural remains were contained in the Black Earth stratum exposed on the ground surface. The density of settlements was fairly large; the distance between settlements only several kilometers. At three of these sites, Sao-ta-kou, West T'uan-shan-tzu, and T'u-ch'eng-tzu, village cemeteries were located containing a large number of cist-graves.

Generally speaking, these sites are of the same stage of culture and belong to the same tradition, but stratigraphically they could be divided into two sub-stages, the later of which is characterized by the presence of a number of cultural items that show influences of the Late Chou and Han cultures to the south.

The prehistoric inhabitants of the Kirin area were most probably primitive farmers. Pig bones were found in cist-graves of all of the cemeteries, and remains of hemp-clothes and cordage were also salvaged. Stone axes, knives, sickles, and grinding stones in good quantity were seen at all of the sites. Seed grains found in pottery of Graves IV and XIV of the West T'uan-shan-tzu cemetery have been identified as wild millet (*Eriochloa villosa* and *Setaria lutescens*), both potential plant domesticates of the area. These evidences seem to indicate that the

13 Mikami, 1939; Li, 1946.
14 Tung, 1957.

inhabitants were at least incipient food-producers, if not already fully established village farmers. Hunting and fishing are shown by the remains of arrowheads and stone and clay net-sinkers. These were, however, very scarce, and totally absent at some sites. The grave goods of the cist-tombs suggested two recurrent combinations: (1) spindle-whorls, stone knives, and pottery; (2) arrowheads, stone axes, stone chisels, and pottery. These might indicate that men hunted and cleared the field, while women wove, attended domestic duties, and possibly weeded.

The stone inventory from these sites consisted of round and flat axes, adzes, knives, sickles, a few daggers imitating metal prototypes, arrowheads, grinding stones, and net-sinkers; some of the axes were flaked, while the rest were all ground. Over ninety-five percent of the potsherds and pottery were plain, brownish, sand-tempered, and occasionally burnished. The few decorations, when they occurred, consisted of cord-marks and incised geometric designs. All were hand-made; sometimes built in sections. The form was characterized by straight wall and flat bottom, but *li*- and *ting*-tripods also occurred.

The cemeteries were thick with graves. At West T'uan-shan-tzu, in an area of 330 square meters were excavated thirty-four individual cist-graves. The graves were about 1.8 m long, 50-60 cm in width, about 40 cm high, and were built of granite slabs. The skeletons were mostly extended; a few were semi-flexed. Grave-goods consisted of ornaments in the chest and abdomen areas, stone implements around the waist and the hips, and pottery near the feet or in a separate cist-pit at the foot-end.[15]

Two localities in the Kirin area will be described as examples.

T'U-CH'ENG-TZU

The Neolithic site at T'u-ch'eng-tzu was situated on the low plain on the right bank of the Sungari River. The occupation was subdivided into two stages: the Lower Stratum, a Neolithic settlement which consisted of a dwelling area and a cemetery, and the Upper Stratum, a continuation (with some spatial shifting) of the same culture with the inclusion of pottery and metal artifacts of the Late Chou and Han Dynasties style.

One dwelling floor was located at the site. It was a rectangular semi-subterranean pit (5.30 by 2.25 m); its floor and walls were beaten and then hardened by fire. Possibly a timber superstructure was built upon the pit. At the southeastern corner of the pit was a pile of gravel, which might have been the remains of a hearth. Other round and oval pits were found, ranging in diameter at the mouth

15 Tung, 1955, 1957; Okuda, 1939.

from 2.3 to 10 m. In them were found potsherds, charcoal, burned clay, and bone fragments. Possibly these were also remains of pit-houses.

To the south of the dwelling area was a cemetery of cist-graves of which twenty-six were excavated. The cists were rectangular, 1.2-2.8 m long, and 0.47-1 m wide, and consisted of granite slabs (for walls) and (questionably) slates (for bottoms and lids). The bones were mostly decomposed. From what were left it might be inferred that the head of the dead was placed toward the east. Grave goods consisted of stone implements and ornaments; metal tools, jade objects, and pottery cups were present but rare. Surrounding each cist, scattered or in piles, were a great many net-sinkers of clay, and on the lid of over ninety percent of the graves were found pigs' teeth, some of which were charred and cemented with burned clay and charcoal.

The stone inventory included twenty-two polished axes (rectangular, round, and square), nine knives (all semi-lunar in shape, with two perforations), four arrowheads, and a few chisels and hoes. Three pottery wares were found at the site: (1) fine-paste ware of small quantity and limited spatial distribution; (2) grey, sand-tempered, hard ware, wheel-made, containing the form of *tou;* these were rare and confined to the Han Period occupation; and (3) red, sand-tempered ware, its characteristic form being the *li*-tripod. Few metal artifacts were found in the Upper Stratum (an iron arrowhead, four bronze rings, a bronze knife, and fourteen bronze ornaments). Bone tools included knives and arrowheads. Faunal remains consisted of pig bones (predominant in quantity) and bones of horses and cattle.[16]

West T'uan-shan-tzu

At this site both Neolithic settlement (dwelling area and cemetery of cist-graves) and Han brick tombs were found. The Neolithic remains resembled those of T'u-ch'eng-tzu. The heads of the skeletons were oriented toward northwest instead of east as at T'u-ch'eng-tzu, but in both places they were directed uphill despite the difference in cardinal direction.[17]

To sum up the Neolithic remains of the Kirin area: The earliest known Neolithic remains discovered here indicate village farmers and cist-grave builders. Their pottery was characteristically reddish and brownish in color, largely devoid of decorations, straight-walled and flat-bottomed. Two ceramic stages are divisible: an earlier one containing tripods of North China style, and a later containing Han Dynasty style grey ware and metal objects.

16 Kang, 1955, 1957.
17 Mikami, 1939; Wang, 1960a.

THE UPPER SUNGARI VALLEY: CH'ANG-CH'UN AREA

On the low hills in the vicinity of the city of Ch'ang-ch'un, along the banks of the I-t'ung River, a tributary of the Sungari, a number of prehistoric sites have been located. These may be classified in two groups: (1) sites that yielded microliths, represented by Shih-pei-ling; and (2) sites whose remains resemble the Kirin Neolithic.

Shih-pei-ling-type Sites

At Shih-pei-ling were found chipped axes of basalt, a few polished, flat axes, quartz discs, mace-heads, and a great number of microliths. The pottery was brownish, sand-tempered, with straight walls, and decorated with incised geometric designs and punctate marks. Both polished stone knives and *li*-tripods that characterize the Kirin Neolithic were conspicuously absent.[18] No less than twelve other sites in its vicinity were listed in 1957,[19] where microliths and pressure-flaked projectile points (particularly arrowheads) were characteristic.

Kirin-type Sites

In the same area where the microlithic sites were distributed, over a dozen sites which yielded prehistoric remains of Kirin Neolithic type have been located. Apart from the fact that chipped stone implements are more abundant here than in Kirin, the general cultural configurations of these two groups of prehistoric settlements look largely alike. Stone hoes, sickles, milling stones, pestles and mortars from the site at Hung-shih-che-tzu[20] and that at Hsin-li-ch'eng[21] indicate that agriculture was probably practiced.

Tung Chu-chen[22] and Wang Yia-chou[23] are both of the opinion that in the Ch'ang-ch'un area one witnesses a mixture of the cultures of farmers (of the Kirin type) of the Sungari valley and that of the nomadic people to the west on the eastern Mongolian steppes which is characterized by the prevalence of microliths. This may well be the case, but it seems to the author that the available evidence rather favors an alternative interpretation, i.e. that these two types of sites may represent two distinct time levels. An earlier period of culture was characterized by the manufacture of microlithic hunting implements and the absence or scarcity of agricultural implements and pottery tripods, and a later one by the diminishing of microliths and hunting and the increase of farmers' inventories. These two types of remains occur unmixed at discrete sites which are distributed in the same area

18 Mizuno and Komai, 1936.
19 Tung, 1957, p. 37.
20 *Idem*, p. 36.
21 Wang, 1960b.
22 Tung, 1957.
23 Wang, 1960b.

among one another. Although stratigraphic evidence is still lacking, it can be concluded on distributional grounds that these two types of sites do not seem to be contemporary.

THE MIDDLE SUNGARI VALLEY

Archaeological information concerning this area is scanty, but recent (1957-59) investigations at a prehistoric locality near Harbin are significant.[24]

The sites were on a small hill, Huang-shan, in the eastern suburb of the city of Harbin. The hill was separated by an erosional ravine (Ta-kou) into two halves, Pei-ch'eng, in the north, and Nan-ch'eng, in the south.

On the western edges of the Ta-kou were collected twelve stone implements, consisting of flakes, points, scrapers, and choppers. They were of sandstone and quartzite, and were crudely chipped. Further investigations at this locality may disclose a preceramic assemblage of great importance. On both sides of the hill, Nan-ch'eng and Pei-ch'eng, were collected stone implements and potsherds. The stones were of flint, jasper, agate, slate, obsidian, chalcedony, and other silicified rocks, and typologically they were of the typical Manchurian microlithic variety. The main types were arrowheads, leaf-shaped projectile points, and scrapers. At the Pei-ch'eng locality was collected a polished jasper bead. The potsherds were brownish or yellowish, hand-made, sand- and shell-tempered. Flat bottoms were again characteristic. Decorations consisted of appliqués, punctate marks, incisions, fingernail marks, impressions and cord-marks, and the designs rectilinear and geometric. At the Pei-ch'eng site was found a section of bird's long bone, 1.2 cm long, possibly another occurrence of a needle-case.

THE MUTANKIANG VALLEY

A number of prehistoric sites have been located in the Mutankiang valley in its course from Kingpai Lake down to the region where it enters the Sungari, but only a few have been reported in sufficient detail. They are briefly described here, from the lower valley upstream.

Wo-k'en-ha-ta [25]

Prehistoric remains were found in 1950 in a cave in Wo-k'en-ha-ta, a low hill in the region where the Mutankiang joins the Sungari. Subsequent excavations disclosed three occupational layers in the cave and, overlying them, four prehistoric burials. Bones of fish, birds, and mammals, ashes and charcoals, charred bones, and potsherds were found in the occupational layers, and potsherds, stone tools,

24 Chao, 1960b.

25 For a brief summary in English, see Cheng, 1959, pp. 153-154.

bone artifacts occurred with the burials. Stone artifacts included polished, long adzes and stone and jade pendants and beads. Potsherds were formed of fine paste; some tempered with mica, hand-made and burnished, decorated with combed geometric designs. The color was predominately brownish, and the form of the flat bottomed varieties. Among the bone objects were a number of rectangular, perforated plates placed around the waist and hips of the human skeletons; they apparently were components of armor. Burials were particularly interesting: the dead were squatting, in a flexed posture, on large flat slabs. These remains indicate an archaic type of hunting-fishing culture, but the absence of microliths and the presence of jade pendants of northern Chinese style are both conspicuous.[26]

Lin-kou Hsien Sites

In the lower Mutan valley in the territory of Lin-k'ou Hsien were located in 1958 eight prehistoric localities. A great amount of potsherds and clay net-sinkers were collected. The sherds were all hand-made, sand-tempered, brownish in color, with little decoration. The shape of the pottery is not clear, but flat bottoms apparently were a characteristic feature.[27]

Ao-tung

The site at Ao-tung was on a riverine terrace south of Mutankiang City in the middle course of the Mutan River. Besides some plain, brownish, sand-tempered, and hand-made potsherds, the presence of polished axes and knives is significant.[28]

Niu-ch'ang

This locality, again on a Mutankiang terrace, was an extensive occupation site and had remains of stone, pottery, bone tools, and fragmentary bones of fish, pig, deer, sheep, small mammals, and turtle. Fish bones were particularly abundant; together with barbed fish-hooks of bone and stone and clay net-sinkers, they attested the importance of fishing among the prehistoric inhabitants of the site. The pottery was hand-made, sand- and shell-tempered, characteristically flat-bottomed, and plain or decorated with incised geometric designs. Stone tools consisted of chipped microliths (arrowheads and scrapers) and polished axes, slate knives, and milling stones. A section of masonry wall of basalt rocks might have been the remains of a dwelling house.[29]

Ying-ko-yü

Hand-made, brownish, sand-tempered sherds with incised geometric designs

26 Li, 1954.
27 Tan and Sun, 1960, pp. 25-26.
28 *Idem*, pp. 23-24.
29 Chao, 1960a.

were collected at this site, together with microliths, bone artifacts, a pig figurine, and burnished red sherds painted in black pigment.[30]

San-ling-t'un

On the terrace near San-ling-t'un village, eight km north of Tung-ching-ch'eng, potsherds and stone implements were collected. The sherds were brownish and greyish, sand-tempered, hand-made, and plain. Stone tools were both chipped and polished. Among the former were mainly microlithic arrowheads, and the latter slate knives and flat axes. Stone net-sinkers, mortars and pestles were also found.[31]

GENERAL DISCUSSION

In ancient northeast Asia, subsequent to the widespread microlithic industries appeared a ceramic phase characterized by pointed-bottomed pottery with impressed decorative designs. This ceramic phase is divisible into two major facies. One centers in the taiga region in interior eastern Siberia, is typified by the textile-impression on pottery, and makes its first appearance in the Isakovo stage of the Baikal sequence. The other is situated along the Pacific coasts, represented by the (hypothetical) Shengwen Horizon of the Huangho valley in north China[32] and the Initial Jomon stage on the Japanese islands, and characterized by the popularity of cord-marks. Although the taiga facies remained little changed in basic cultural style for a long time, subsequent developments in the maritime regions led to the predominance of flat-based pottery and a number of other cultural and developmental innovations which eventually brought about a great range of cultural complexity. One of the most important events that took place in this area was certainly the development of Neolithic cultures and historical civilizations in north China[33] which affected profoundly the various sub-areas in their cultural forms and developmental patterns through diverse historical courses.

Situated at the meeting place of the major Neolithic centers in northeast Asia and easily accessible from all, the Sungari valley probably played an important role. It appears, therefore, advisable at this stage to summarize our understanding of the cultural sequences of prehistoric inhabitants in this area, although we can only do this in an extremely tentative manner as its archaeological record is still incomplete.

30 Tan and Sun 1960, pp. 24-25.

31 Komai, 1934; Komai and Mikami, 1936.

32 Chang, 1959a.

33 For a recent synthesis of Neolithic development in the Huangho valley, attention is called to several papers by the present author (Chang, 1959a,b, 1960) which differ from the traditional interpretation in a number of respects.

I. Preceramic Stage

For preceramic sites in the Sungari valley there is the famous but highly controversial industrial assemblage from the Black Earth stratum at Ku-hsiang-t'un, near Harbin.[34] Here Tokunaga and Naora unearthed a small group of chipped implements fashioned in "typical Gobi microlithic technique," in association with a typical Late Pleistocene fauna.[35] Although its geological antiquity has been rightly questioned,[36] the Ku-hsiang-t'un stone and bone industry, together with the recent finds at Ta-kou, in the same neighborhood, indicates that the Sungari valley was inhabited at least from early post-Glacial times by hunter-fishers of a microlithic culture tradition.

II. Sub-Neolithic Stage

The earliest ceramic phases of northeast Asia, characterized by pointed-bottomed vessels of either the cord-marked or the textile-impressed varieties, have not been represented by archaeological materials from the Sungari valley up to this time. The first ceramic phase in this area is represented by the finds at Anganghsi and Hsiao-miao-tzu in the Nonni valley; Shih-pei-ling and other Ch'ang-ch'un I sites in the upper Sungari; Nan-ch'eng and Pei-ch'eng near Harbin in the middle Sungari; and Wo-k'en-ha-ta, Lin-k'ou Hsien sites, and Ying-ko-yü in the Mutankiang valley.

Cultures of this stage are characterized by the following stone, bone, and ceramic traits. Microliths (arrowheads, scrapers, and side-blades) and pressure-flaked leaf-shaped points are abundant in quantity, probably indicative of the importance of hunting. Some quantity of polished stone tools, mainly axes and adzes, are present, in addition to elaborately made ornaments, but polished slate knives are conspicuously rare, if not absent. For bone artifacts, there are spear-heads, fish-spears, toggle harpoons, armor plates, and needle cases of bird bones. Pottery is predominately brown in color, sand- or shell-tempered, flat-based and with a straight-walled body. It is decorated with incisions, fingernail impressions, cord-marks, and punctate marks, and the designs are without exception geometric. Some repetitive motifs include parallel lines, zigzags, lozenges, and triangles. Bearers of the culture of this stage were hunter-fishers, perhaps with a stress on hunting, and evidence of agriculture is extremely meager, if not non-existent.

With Neolithic cultures of the Huangho and their derivatives, those of the Liaoho valley, the Sungari Sub-Neolithic shares a number of stylistic elements.

34 For brief summaries in Western languages, see Cheng, 1959, pp. 44-46; Teilhard de Chardin, 1941, pp. 81-82; Teilhard de Chardin and Pei Wen-chung, 1944, p. 46.

35 Tokunaga and Naora, 1933, 1934.

36 Teilhard de Chardin, 1941, p. 81.

Flat-bottomed and vertical-walled vessels are as characteristic of the Yangshao and especially Lungshan stages of the Huangho Neolithic as they are here, and likewise are the black-on-red ceramic painting that was found in the Mutankiang valley and the cord-marked pottery. The geometric designs find many striking parallels among the ceramic wares of the Lungshan stage of the Huangho. The jade ornaments of Wo-k'en-ha-ta, as Li has pointed out, are probably imports from northern China.

However, even closer are the similarities in the Sungari finds to the "Neolithic" stage of the Maritime Province of the U.S.S.R. near Vladivostok[37] and that of the lower Amur at Suchu Island and Kuinga.[38] Items that show close Maritime affinities are the following from the Sungari basin: flat-based and vertical-walled pottery as an almost exclusive ceramic shape; ceramic painting, incisions, and pit or comb indentations; bone armor plates; bone needle cases; and toggle harpoons. It does not seem to be an exaggeration to state that the cultures of the Sub-Neolithic stage in the Sungari valley were the interior facies of the cultures of the Pacific coast. In terms of its decorative designs, the ceramics of this area seem to be closer to the Vladivostok assemblages than to the lower Amur.

On the other hand, one cannot be sure whether the geometric patterns of ceramic decorations in the Sungari were not in some way linked to the ceramic decorative fashion of the taiga inhabitants around the Lake Baikal area. Together with this, the Sungarian microlithic forms, particularly the side-bladed bone knives and the Anganghsi long points with retouched tips, and stone adzes with a trapezoidal cross-section of the Wo-k'en-ha-ta cave show remarkable linkage with the upper Angara Neolithic, although the ceramic shapes in those two areas are largely different.

III. Neolithic Stage

The next stage in Sungarian prehistory witnessed the introduction of agriculture and many decisively Huangho-style cultural traits in the upper Sungari valley and the influence of the same cultural impact, if not food-production in its complete form, in the more distant and less favorable parts of the Nonni and Mutankiang valleys. Typical of this stage are the prehistoric localities at Hung-shih-che-tzu, Hsin-li-ch'eng, and other Ch'ang-ch'un II sites, T'u-ch'eng-tzu I and other Kirin I localities, in the upper Sungari; Kuan-ti-ts'un and Hsi-nan-ti-ken-ts'un in the Nonni valley; and Ao-tung, Niu-ch'ang, and San-ling-t'un in the Mutankiang valley.

Microliths apparently diminished in number, possibly indicating that hunting

37 Okladnikov, 1958, pp. 550-552.

38 Chard, 1958, p. 23; Field and Prostov, 1942, p. 396.

as a means of subsistence was no longer predominant. Taking their place were polished stone axes and adzes, which increased in quantity, and semi-lunar knives and sickles, some of which were significantly made of slate. Evidence of agriculture is indisputable in the upper Sungari sites in the Kirin and Ch'ang-ch'un areas. In the Nonni and Mutankiang valleys evidence of farming is still indirect, but the importance of fishing had apparently increased. Of the ceramic wares, the indigenous tradition of flat-based pots and cups still predominated in frequency of occurrence and in geographical distribution, but now, especially in the upper Sungari, *ting*- and *li*-tripods of typical Yellow River design made an impressive debut. The color continued to be predominantly brownish, but there is also a considerable amount of reddish and greyish wares. The surface of the ware is in most cases plain, but incised geometric designs still occur. A significant addition is the engraved geometric pattern seen at Kuan-ti-ts'un.

It can be observed that upon the substratum of a maritime tradition now fell a heavy Yellow River cultural influence. Food-production, with millet and pig as characteristic features, seems to have come from that direction. So do hemp cultivation, pottery tripods, and some pottery designs (noted especially in the lozenges of Kuan-ti-ts'un). But communications with the taiga and the Maritime province apparently continued, as indicated by the semi-subterranean dwellings, the cist graves which are characteristic of the Baikal area, and by the flat axes and stone daggers (which are rare, however, in our area and may actually be of the next stage) that are typical of the Vladivostok and northern Korea Shell-Mound Culture and the assemblages of the Tumen valley in southeastern Manchuria.[39]

IV. Chalcolithic Stage

The Sungarian inhabitants in a next stage apparently carried on their older (Neolithic) way of life, but in the upper Sungari regions at least many northern Chinese cultural items of Late Chou and Han Dynasties date were imported. The socketed, fan-shaped axes and knives of Sao-ta-kou, Kirin, and other metal objects and jade artifacts of the Kirin area, find close affinities at Mu-yang-ch'eng site in the Port Arthur region, the Hung-shan-hou II (cemetery) at Ch'ih-feng in eastern Mongolia, and the late Chou sites near T'ang-shan, Hopei. These finds all range chronologically from the late Warring States Period to the Han Dynasty. By the middle of Western Han, the Liaoho valley was completely sinicized,[40] and the Western Han civilization, as is well known, had been established in northern Korea. It is only reasonable that in the area immediately to the north, in the upper Sungari valley, should strong influences of the Han civilization be directly felt.

39 Kang, 1956; An, 1948; Tung, 1957.

40 Tung, 1956.

Since the last stage of Sungarian prehistory can be accurately dated, an attempt at a chronological alignment of the whole sequence should not be too wild an effort. Since the Chalcolithic Stage of the upper Sungari contained Late Chou (722-221 BC) and Western Han (206 BC-AD 8) characteristics, it may be said to have started about the middle of the first millennium BC. The Sub-Neolithic stage, on the other hand, started with flat-based ware and, therefore, it might not have begun until after the stage when the pointed-bottomed vessels prevailed in Japan (Initial Jomon, C-14 dated back to the 6th or 7th millennium BC) and in the Baikal area (Isakovo, estimated by A. P. Okladnikov as of the 4th millennium BC). Since Okladnikov's dates of the Lake Baikal sites are rough estimates pushed back from the Kitoi stage,[41] and the Initial Jomon's C-14 dates, derived from shell, are open to doubt,[42] it seems more proper to regard the beginning of the Sub-Neolithic stage of the Sungari valley as no earlier than the 5th millennium BC when according to reliable C-14 dates the flat-based vessels made their appearance in Japan (Early Jomon).[43] Therefore, in the Sungari valley, the Sub-Neolithic and the Neolithic stages may be placed within the somewhat long time-span from the 5th to the 1st millennium BC in the upper Sungari valley, while in other places they might have lasted much longer, through the beginning of Han or even later. It is difficult to determine at what point in this temporal span might the Neolithic stage have begun in the Sungari valley, but it can safely be said to be subsequent to the beginning of the Lungshan stage in the Huangho Neolithic when pottery tripods and fruit-stands became the predominate forms of ceramic vessels.

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41 Okladnikov, 1950, pp. 117-140.

42 Chang and Beardsley, 1959, p. 9.

43 *Ibid.*

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ON THE ANALYSIS OF RITUAL

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IN A RECENT ARTICLE in this journal Mischa Titiev offered what he called "A Fresh Approach to the Problem of Magic and Religion."[1] As the subject of religion is one that has been much neglected by anthropologists in modern times, a new approach should be quite welcome. I do not feel, however, that the article fulfills the promise of its title, and I would like to discuss briefly my objections to it.[2]

Titiev proposes to substitute a dichotomy between "calendrical" and "critical" rites for the traditional one between "religion" and "magic" in the study of man's relation to the supernatural world. He does so because he feels that the dichotomy between "magic" and "religion" has proved unsatisfactory, yet that some such distinction is needed for the purpose of classification and analysis "unless we are prepared to lump all supernatural phenomena into a single category."[3] "In all primitive societies one set of practices involving the supernatural always takes place recurrently," he says, "and may accordingly be termed *calendrical;* while the other set, which is celebrated only intermittently, and then only when an emergency or crisis seems to have risen, may be called *critical.*"[4]

He further finds that ". . . calendrical observances may be said always to have a 'church,' and to correspond, on the whole, more nearly to established concepts of religion than to those of magic," whereas "for the most part critical ceremonies are staged only when a private or personal emergency has arisen."[5] The latter, therefore, do not need a "church," and "since, at the same time, they do not necessarily have to be performed by socially sanctioned priests, they tend in a number of ways to correspond quite closely to traditional concepts of magic."[6]

Finally, considering the universal existence of some form of calendar basic to his distinction, he attempts to show that it is a simple matter for "even the most primitive and non-literate of people" to keep track of the course of the seasons.[7]

Now although objections can be made to the terms used by Titiev, and I shall do so below, the first thing that strikes one about the dichotomy he proposes is that

1 Titiev, 1960.

2 I am indebted to Dr R. F. Gray of the University of Illinois for much helpful advice in the composition of this article.

3 Titiev, 1960, p. 297.

4 *Idem*, p. 293.

5 *Idem*, p. 294.

6 *Idem*, p. 295.

7 *Idem*, p. 295f.

it is by no means the first time that it has been applied. Junod, for example, in describing the religion of the Thonga of southeast Africa, distinguished between what he called "regular offerings" and "those made in special circumstances," as one of a series of distinctions.[8] Nadel also, in his analysis of Nupe ritual, made the same basic division, though using the terms "fixed" and "movable": "On the one side we have rituals occupying a regular, fixed place in the calendar of the country; on the other, rituals which, as the Nupe say, 'have no month', that is, whose performance is contingent upon the variable needs of the moment."[9] More recently I made use of the same distinction myself in a study of the religion of an Indonesian people, where I used, however, the terms "regular" and "contingent."[10]

I preferred to speak of "regular" rituals because among the people whose religion I was describing, none of the rituals which fitted this category, except those concerned with agriculture, was performed at fixed times of the year. Head-hunting, initiations, and funerals for the bones of the dead were held more or less regularly, but the exact times for their performance were determined by a variety of circumstances. The basic idea of this classification, it seems to me, is the fact that there are rituals whose repetition at more or less regular intervals is felt to be necessary for the maintenance of the society or which sanctify regular social activities. Their regulation by a fixed calendar, however, is only incidental unless they are specifically related to natural phenomena, and it is therefore not necessary to demonstrate that all societies must have a way of establishing such a calendar.

I would also choose "contingent" in preference to "critical" on two grounds. In the first place "calendrical" and "critical" are not logical opposites in their ordinary meanings. Secondly the use of "critical" invites its confusion with so-called "crisis rites," which Titiev in fact places in his "critical" category, though admitting that they are difficult to classify,[11] whereas I would consider them to be primarily "regular." It will also be remembered that Van Gennep placed those rites accompanying the change of the year, season, or month among his rites of passage.[12]

My second reaction to Titiev's proposal is that it does not really constitute an "approach to the problem of magic and religion" at all. This problem apparently consists for him in the fact that it is difficult if not impossible to distinguish clearly between the two types of phenomena and that it is therefore impossible to use these two categories as a basis for classifying and analyzing supernatural phe-

8 Junod, 1913, vol. 2, p. 362.
9 Nadel, 1954, p. 68.
10 Downs, 1956, p. 100.
11 Titiev, 1960, p. 297.
12 Van Gennep, 1960, p. 178.

nomena as a whole. By using a dichotomy of a different type, however, based on the occasions on which rituals are performed, he adds little if anything to our understanding of the relationship between religion and magic and by no means renders their distinction useless. In spite of the difficulty in clearly separating the two categories with respect to the supernatural activities of a particular society, the contrast between attempts to communicate with and control the supernatural either "through appeals and prayers to some transcendental intelligence (in the manner of religion) or quasi-mechanically, by the manipulation of 'magic' agencies," as Nadel put it,[13] remains significant, and it cannot be fully comprehended in or explained by Titiev's categories of "calendrical" and "critical" rites.

The point is that both "religion-magic" and "regular-contingent" are valid dichotomies which are neither coterminous nor mutually exclusive. Nor are they the only ones possible, for it is certainly not true that we must choose any one as the only alternative to putting all supernatural phenomena into a single category. Evans-Pritchard, for example, distinguishes between "collective" and "personal" sacrifices among the Nuer, subsuming respectively those sacrifices which accompany various social activities involving social groups and those made for a person or persons,[14] and Hubert and Mauss spoke of sacrifices of "*sacralisation*" and "*désacralisation*," in which the primary aim of the first is to obtain the beneficial effects of the sacred, whereas that of the second is to gain release from its evil effects.[15] All of these classifications bring out different aspects of ritual, based on distinctions respecting the means employed, the occasions on which the rites are performed, for whom and for what purpose.[16]

None of these distinctions, however, is any more absolute than that between magic and religion, and considerable overlapping is to be expected. Rites of passage obviously have both collective and personal aspects, since they involve the regulation of social statuses and relationships basic to the society as a whole that practices them, yet they are performed for individuals. As for the distinction made by Hubert and Mauss between sacrifices of "*sacralisation*" and "*désacralisation*," they found that although all sacrifices involve these two processes, their relative importance varies according to the intent of the particular sacrifice.[17] It also seems

13 Nadel, 1954, p. 3. The same distinction was also made by Benedict (1938, p. 637). I do not consider the equation of magic with religious techniques as a whole, as proposed by Van Gennep (1960, p. 13) and adopted by Chapple and Coon (1942, pp. vii, 529) to be useful. Why, incidentally, the latter attribute this "discovery," as they call it, to A. M. Tozzer I cannot understand.

14 Evans-Pritchard, 1956, p. 198.

15 Hubert and Mauss, 1899, p. 89ff.

16 Even more such categories are, of course, possible. Cf., for example, Mauss, 1947, Ch. 9.

17 Hubert and Mauss, 1899, p. 89ff.

that the "fixed" and "movable" categories of Nupe ritual are open to variation, for we are told that some of the former can be performed out of season and that some which were originally "fixed" have become "movable."[18] The Toradja, moreover, felt that the regular taking of heads was essential to the life of the community, but there were also particular occasions for it as well, and certain rituals enacted regularly in connection with agricultural activities were often performed in response to particular situations. *Moandu sala,* "to float (the) evil away," for example, was carried out annually before the preparation of the rice fields to cleanse the village of its sins, but it was also used as a piacular offering for incest in cases of normally forbidden marriages.[19]

It can also happen that two or more of these sets of distinctions tend to coincide in particular instances, though generalizations concerning such coincidences would appear to be of limited validity. Evans-Pritchard, for example, considers that the "personal" sacrifices of the Nuer have primarily a piacular intention, whereas the "collective" sacrifices have a confirmatory one, a distinction which he considers equivalent to that of Hubert and Mauss between rites of "*désacralisation*" and "*sacralisation.*"[20] Similarly Titiev finds that his "calendrical" and "critical" rites (which I would call "regular" and "contingent") tend to correspond to "communal" and "private" rites respectively.[21] One might be tempted to conclude from this that all these categories were in agreement and that a single pair of terms could comprehend all the others.

Unfortunately, however, Evans-Pritchard does not include the same kinds of rituals under "collective" as Titiev does under "calendrical" rites. "The primary purpose of collective sacrifices," he says, "and also their main function, is to confirm, to establish or to add strength to, a change in social status—boy to man, maiden to wife, living man to ghost—or a new relationship between social groups —the coming into being of a new age-set, the uniting of kin groups by ties of affinity, the ending of a blood-feud—by making God and the ghosts, who are directly concerned with the change taking place, witnesses of it."[22]

Now it is immediately apparent that all of the sacrifices to which Evans-Pritchard here refers would be placed by Titiev in his "critical" category, in spite of the fact that it is the "calendrical" rites which he considers to be communal in character. This discrepancy could be partly eliminated by changing Titiev's "calendrical" to "regular" and putting the rites of passage in this category, as I

18 Nadel, 1954, p. 68.
19 Downs, 1956, pp. 92, 100.
20 Evans-Pritchard, 1956, p. 198f.
21 Titiev, 1960, p. 294.
22 Evans-Pritchard, 1956, p. 199.

suggested above,[23] but Evans-Pritchard's final example, the sacrifices accompanying the ending of a blood-feud, could not be so classified. They are clearly not periodic in nature, and they constitute, furthermore, only one example, which could easily be multiplied, of rituals performed contingently or in times of unexpected crisis which are the concern primarily of the group rather than of the individual. Similarly there are rites regularly or even "calendrically" performed by individuals.

Finally, although rites of passage can be classified in relation to "regular" and "contingent" or "collective" and "personal" categories, Nadel, basing his classification of Nupe ritual on the attitude of the Nupe themselves, placed their ceremonies attendant on birth, marriage, and death in an entirely different category. It appears that they are not regarded by the Nupe as *kuti*, or rituals proper, but merely as "ways of doing things." Because they are connected with the whole system of religious ideas and involve appeals to God, however, Nadel referred to them as "*solemnizations* of mundane acts" as opposed to rituals proper, both "fixed" and "movable", "which wield supernatural power for particular ends."[24] Of course these "solemnizations" are rituals from the anthropologist's point of view and could be put quite conveniently under "collective," "regular," or "confirmatory" headings, but such classifications would fail to bring out a basic distinction in Nupe religious attitudes.

In conclusion I would summarize the points I have been trying to make in the course of these brief comments as follows:

1. A dichotomy of the type Titiev proposes, though using different terms, is significant and useful.

2. It cannot, however, serve as a substitute for one between religion and magic.

3. These are only two of many possible dichotomies which can be used, and no one of them is adequate in itself to a complete analysis of ritual.

4. Since such categories are not watertight compartments, as it were, into

23 It seems to be that Titiev's classification of these rites is wrong for a variety of reasons (cf. Titiev, 1960, p. 297). He considers those connected with birth, marriage, and death to be "critical" first because they are concerned primarily with individuals, yet we have seen that the communal aspects of at least the latter two are equally if not more important than the private ones. Moreover, his "individual" criterium belongs to another dichotomy which need not always coincide with the one he is applying. He also considers these three groups of rites "critical" because they cannot be precisely determined in advance, whereas puberty rites may be celebrated "calendrically," but I have indicated above why I do not believe precise dating to be essential. He finally considers all rites of passage to be "critical" because "they drastically change a person's social relationships," which is the very reason Evans-Pritchard gives for considering them "collective." The crises involved here, moreover, are ones which are part of a series of regular, normal events in the life of a society and its individual members.

24 Nadel, 1954, p. 115.

which all religious practices can be fitted, but are only conceptual poles between which all degrees of variation are possible, they should not be rigidly applied.

It follows from the above that the application of several such dichotomies to the analysis of the ritual of a particular society, showing the distribution of the practices in question among the various categories and the relation of the latter to each other, could serve to reveal and evaluate the relative importance of various aspects of the religion of that society.

It should perhaps be emphasized, however, that this paper has only been concerned with the analysis of ritual and that the various terms discussed are only intended for application to it. They are by no means to be seen as a key, however imperfect, to the analysis of all religious phenomena, as Titiev seems to imply in the case of his own classification. Obviously the religious practices of any society must be seen in relation to its beliefs if they are to be fully understood. The latter, however, must be analyzed in different terms.

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THE SURVIVAL OF THE DART-THROWER ON THE PENINSULA OF BAJA CALIFORNIA

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INTRODUCTION

THE SPEAR or dart-thrower is a hunting and war weapon which has a similar history in all parts of the world. It is old—as attested by the Magdalenian specimens from the Upper Paleolithic of western Europe which date from about 20,000 years ago. Generally it has had a world-wide distribution in Europe, Asia, North and South America, Australia, Melanesia, and Micronesia. It has subsequently dropped out of the cultural inventories of most Eur-Asiatic and New World peoples; this has apparently been through replacement by the bow and arrow, or other superior weapons. Here and there the dart-thrower continued in use, up until the time of the great 16th century European overseas expansion, under three historical conditions: (1) survival among groups peripheral to the main continental cultural currents (Australia and New Guinea and perhaps Brazil); (2) use or retention as specialized equipment for hunting waterfowl (Eskimo, Northeast Asia, and parts of Mexico); and (3) as a traditional retention among the advanced peoples of Nuclear America (Meso-America and Peru).

In the New World the dart-thrower has come to have particular significance for the archaeologist and the ethnologist. It appears probable that many of the earliest hunting cultures were equipped with this weapon as well as the simple spear. That the dart-thrower had wide usage is borne out by its early stratigraphic position in the archaeology of various areas of North America where it may be represented by the entire weapon or remains of its constituent parts. At a relatively recent date the bow and arrow appeared in North America, probably as a result of diffusion from Asia and the Old World and possibly as late as 500 AD. The bow and arrow then rapidly supplanted the dart-thrower except for certain areas under the general historical conditions as outlined. The result was that few peoples were using the dart-thrower at the time of European contact in the New World.

The remains of this weapon are so rarely recovered, and the ethnographic accounts of its use are so infrequent in North America that careful analysis of all data pertaining to its temporal and geographical position is warranted. The importance of the discovery of complete specimens in Baja California is enhanced by documentary evidence that this weapon was still in use there during the 17th

century, only to disappear from native culture by the following century. This short paper concerns the circumstances of the occurrence and disappearance.

THE ABORIGINAL PENINSULA

Baja California occupies a unique position in the New World. Geographically it is isolated not only by its position but also by its extremely arid climate and its difficult terrain. Inhabitants, both animal and human, maintain themselves in relative isolation, sheltered from outside influence. So far as the Indians were concerned, cultural innovations infiltrated the peninsula slowly. In most instances, the old traditions and ways of life remained long after changes had occurred among distant neighbors to the north and across the Gulf.

The Cape region in the extreme south of the peninsula, where the dart-throwers are reported archaeologically and ethnographically, was inhabited in historic times by now-extinct Guaicurian-speaking Indians who were bordered on the north by Yuman-speakers (Fig. 1). These southern groups include the Pericú, Cora, Huchití, and the Guaicura. They shared certain cultural traits and complexes with their Yuman-speaking neighbors immediately to the north in central Baja California: a hunting, fishing, and gathering economy; band organization; religious concepts; types of watercraft; and certain negative traits such as lack of the dog and pottery. However they differed from the Yuman-speaking groups in language, socio-political organization, and in the simplicity of many of their material traits.[1] Such aspects can largely be explained by the geographical and cultural isolation of the extreme south. Against this culture-historical background, the prehistoric and historic presence of the dart-thrower has meaning.

THE ETHNOGRAPHIC DART-THROWER

Spanish documents of the 17th century mention the historic use of the dart-thrower (*atlatl, estólica, tiradero, lanzadardo*) by Indians of the Cape region and by the Pericú *Isleños* of the off-shore Gulf islands (Fig. 1). There are no known accounts which attribute the weapon to any of the Yuman groups of central and northern Baja California from the earliest description of Ulloa (1539) down to the present (Massey, ms.). Even for the southern part of the peninsula there is no mention of the dart-thrower after 1642.

The Spanish were attracted first to the southern Cape region by the discovery of the peninsula there in 1534, when part of the ill-fated Becerra Expedition sent

1 Kirchhoff, 1942; Massey, 1949; Driver and Massey, 1957.

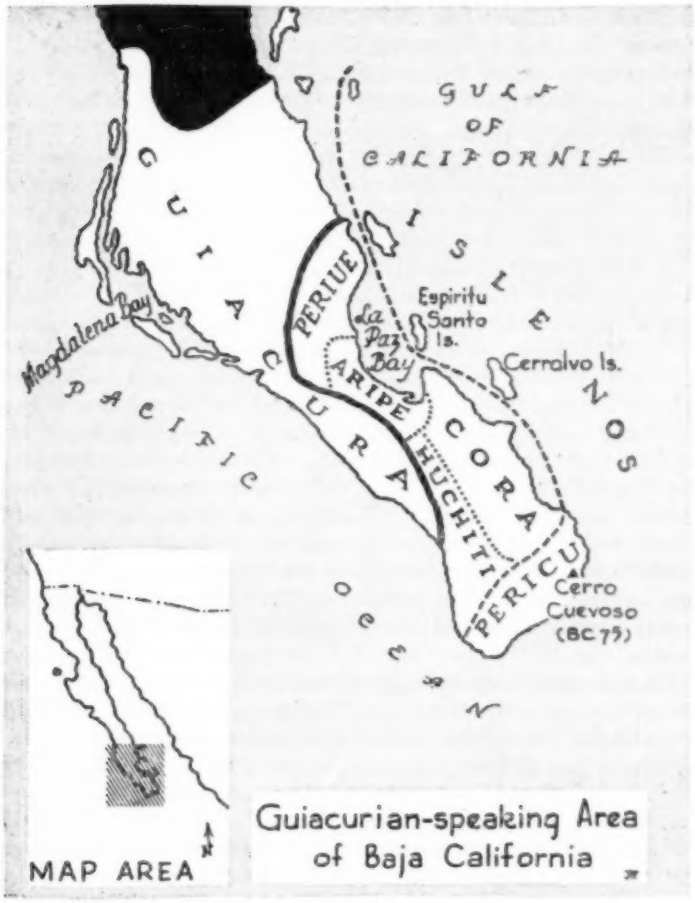

FIG. 1. Tribal locations in southern Baja California.*

out by Hernán Cortes landed at the Bay of La Paz. A year later the Conqueror of Mexico himself was lured to the bay by the reported wealth in pearl fisheries.

* Read *Guaicura* for *Guiacura*.

His attempt to establish a colony there was a failure, as were similar enterprises subsequently.

Following the attempt of Cortes to establish a colony in the south there were a series of expeditions which contacted the peninsula and its inhabitants. These include the following up to 1697, when the Jesuit missionaries took over the peninsula: the coastal exploration of Francisco de Ulloa (1539); the short lived colony of Sebastián Vizcaino at La Paz Bay (1596); the brief stop of Vizcaino at Cabo San Lucas (1602); the pearl-fishing voyages of Juan de Iturbe for the Cardona Company (1615 and 1616); the three exploratory voyages of Francisco de Ortega (1631-1636); a survey of the pearling beds by Luis Cestín de Cañas (1642); the pearling expedition of Bernal de Pynadero (1664); and finally the attempted colony of Admiral Atondo and Father Kino at La Paz Bay (1683).

Many of the reports for the period between 1535 and 1697, when the peninsula was turned over to the Jesuits, contain information on the use of the dart-thrower by the natives, or of the wooden darts with fire-hardened tips which seem to have been associated with the throwers. There is no such evidence for the use of the dart-thrower from central and northern Baja California for this or later periods.

It is noteworthy, in the south of the peninsula, where spear-throwers are reported, that they are described only from the early 17th century, prior to Jesuit missionization of the Cape region which began in 1720 with the foundation of Mission El Pilar de La Paz. An intensive search of the voluminous descriptive ethnographic material of the mission period (1697-1768), when the native population was declining to extinction, has revealed no mention of the dart-thrower or of the darts.

The earliest reference to the dart-thrower itself is from a document of the Cardona Company which held a grant from the Spanish government to exploit the pearl-fisheries in the Gulf of California. Cardona Company ships under the command of Juan de Iturbe made voyages to the peninsula in 1615 and 1616. In describing the implements and goods seen among the Indians of the Bay of La Paz, Nicolás de Cardona states: *"Sus armas son arcos y flechas y estólicas...."*[2] A footnote to this citation in the *Documentos Inéditos* defines *estólica* as: *"Varas que se tiran encajadas en un palo de dos palmas, y con aquel la arrojan y va mas furiosa que una flecha"*—"Shafts inserted in a stick of two palms [in length], which, when they are thrown, go more rapidly than an arrow." The word *estólica* is a standard term in South America for the dart-thrower, just as the term *atlatl* is in Middle and North America.[3]

2 *Documentos Inéditos* vol. 9, p. 35.
3 Friederici, 1947, p. 247.

The second account is that of Don Luis Cestín de Cañas, governor of Sinaloa. In 1640 he was ordered to survey the pearling grounds of California; the actual voyage took place two years later. His report is quoted at length by the chronicler of early Sinaloa, Father Pérez de Ribas. There is sufficient internal evidence in the report to establish the location of Indians seen using the dart-thrower as either Cerralvo Island or Espiritu Santo Island, both outside of the entrance to La Paz Bay. Cestín de Cañas affirms the fact, frequently noted by 17th century voyagers, that the inhabitants of the islands were in constant warfare with the mainland Guaicura.[4] In his description he states that the islanders used the dart-throwers in addition to the bow and arrow: *"Tábien usan de unos dardos tostados, q̄ arrojan con un instrumēto, con que los hazen bolar como flecha"*—"They also use some fire-hardened darts, which they throw with an instrument, with which they make them fly like an arrow."[5]

These two accounts specifically mention dart-throwers. In addition there are frequent references to darts (*dardos*) in use among Indians in the south of the peninsula in the 17th century. Spanish chronicles conspicuously distinguish darts from both spears and arrows. Although such citations are not sufficient in themselves to establish the dart-thrower, they are valuable here as additional information on the distribution of the weapon. The following paragraphs contain all known references to darts for the entire peninsula for the Spanish period.

Darts are mentioned several times in early sources describing the Indians inhabiting the bays and islands of Magdalena Bay. In 1602 Vizcaino cited "small fire-hardened darts" in use at Magdalena Bay,[6] and "wooden darts used also in fishing" at nearby Santa Marina Bay.[7]

For the immediate vicinity of Cabo San Lucas there are two accounts which refer to "darts": Father Ascension (1602) mentions "darts" at San Bernabe Bay[8] and Ortega noted Indians *"cargados de flechas y dardos"* at Cabo San Lucas in 1632.[9]

For the Gulf of California there are two references: that of Cestín de Cañas (1642) for the islands which form part of La Paz Bay, and an account of Ortega's third voyage (1636) which describes the weapons of Indians on the island of San Ildefonso as *"Las armas son arco y flecha y dardos arrojadizos de madera dura."*[10]

4 Massey, 1949, p. 283.
5 Pérez de Ribas, 1645, p. 442.
6 Bolton, 1916, p. 59.
7 *Idem*, p. 60
8 *Idem*, p. 113.
9 *Col. Doc. Hist. de los Viajes*, 1944, p. 82.
10 *Idem*, p. 107. San Ildefonso Island is north of the Cape Region near Isla San Marcos and present day Santa Rosalía.

THE ARCHAEOLOGICAL DART-THROWER

In the Spring of 1947, while excavating a small burial cave on Cerro Cuevoso near Cabo Pulmo (Fig. 1), four dart-throwers were found tied together in a bundle beside a primary burial in the deepest section of the cave. The throwers were enclosed with the burial by a sewn palm fiber mat.[11] These specimens (UCMA 3-11765-68) are in the Robert H. Lowie Museum of Anthropology of the University of California (Berkeley). The cultural remains from this cave have been assigned to the Las Palmas Culture of southern Baja California.[12]

These four Las Palmas dart-throwers, which are among the most remarkable specimens found in North America, are all of the same type although only one is completely intact (Fig. 2). They are all of comparable dimensions. In form this

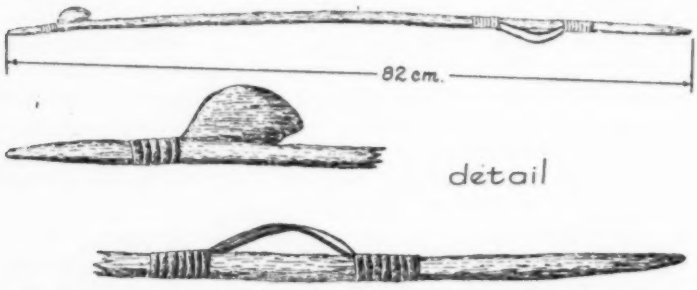

FIG. 2. Las Palmas dart-thrower, showing detail of the hook and grip (Cerro Cuevoso Site BC 75).

type is a slender, long round shaft measuring 82 cm in length and 1.1 to 1.3 cm in diameter. Rising above the distal end of this shaft, and carved from the same piece of hardwood, is a highly polished and rounded tapering hook with a slight projecting prong to receive the concave butt of the dart. On the proximal end of the shaft a single bark loop, attached with sinew wrapping, is set at a right angle to the plane of the hook and to one side of the shaft. The sinew wrapping is stained red. An additional sinew wrapping was used to strengthen the base of the hook. No weights were used with this thrower.

The Las Palmas dart-throwers are classic examples of Krause's "male" type of thrower with the raised hook carved from the same piece as the shaft. They are distinguishable from the "female" type which lacks the hook, and which has a deep channel for the dart which is engaged at the butt end by the terminus of

11 Massey, 1947, p. 349.

12 Massey, n. d.

the channel. Furthermore the "male" type is distinct from the "mixed" type which incorporated the channel of the "female" type with a projecting hook at the end of the channel.[13]

In summarizing recent information on the distribution of types of dart-throwers in the world, Krieger[14] outlines six areas as follows:

(1) Australia: only "male" types;

(2) Melanesia and Micronesia: only "female" types;

(3) Arctic (northeast Asia to Greenland): "mixed" types with some "female" examples on Greenland;

(4) Mexican Area (Oregon to Florida and Panama): "mixed" types;

(5) South America (Panama to north Chile and east to the Rio Xingú): "male" types, with east and west sub-types;

(6) Upper Paleolithic of Western Europe: "male" types.

On the basis of the classification of Krause and the areas as outlined by Krieger we are faced with several disjunctive distributions of "male" types of throwers both in time and space. However, we need be concerned here only with the New World distribution and occurrences.

Since publication of the distributional study of Cressman and Krieger, several archaeological examples of "male" dart-throwers have been reported in the Southeast and in the western Great Basin.

Archaic horizons in the Southeast have been reported to contain compound "male" throwers from an increasing number of sites in the lower Mississippi valley,[15] in Kentucky on the Green River,[16] and in the Pickwick Basin of Tennessee, northern Alabama, and Mississippi.[17]

The type of thrower, which is well-established for sites in these areas, consists of a "male" hook carved from antler, a stone weight (*bannerstone, birdstone*, etc.), and frequently a carved antler handle. In such a thrower the various parts are believed to have been joined by a round wooden shaft. While these dart-throwers of the eastern Archaic have few details in common with the Las Palmas specimens, they do demonstrate that the round shafted "male" type has existed at early time levels in North America (1000 BC–100 AD). An antler hook, such as those reported from Kentucky, was also found in the Shumla Caves of Texas characterized otherwise by the "mixed" type.[18]

13 Krause, 1902.

14 Cressman and Krieger, 1940, pp. 26-38.

15 Jennings, 1952, p. 258, fig. 138,*e*.

16 Webb and Haag, 1939, pp. 50-58, fig. 23; Webb, 1946, pp. 319-333, fig. 55.

17 Webb and DeJarnette, 1942, pp. 270-286, plate 298, 306.

18 Martin, 1933, p. 29, plate IX.

At a much later time-level "male" throwers have been recovered from refuge and marginal areas of the Southeast, much as they have in Baja California. A single round-shafted thrower derives from the Allred Site of the Ozark Bluff-Dwellers. The hook on this specimen was carved from the same piece of wood as the shaft. However, the grip was formed by inserting a short stick transversely through the shaft at the proximal end.[19] This Bluff-Dweller example appears to have been crudely worked compared to the Las Palmas specimens.

In the extreme south of Florida, at Key Marco, Cushing found two wooden dart-throwers, both of which have hooks carved from the same piece of wood as the shaft, which is round on the distal end but flat and broad on the proximal end. The grip was formed on one specimen by a single large hole in the flat proximal end, while on the other there are two holes side by side. Both of these specimens are understandably much more elaborate than the Baja California examples; for instance the hook on the specimen with a single-hole grip is carved to represent a rabbit of which the tail forms the prong to receive the butt of the dart.[20] Dart-throwers have also been reported from south Florida (Calusa) in Spanish times.[21] Chronicles of the De Soto expedition mention the weapon in use near the mouth of the Mississippi River in 1543.[21a]

In western North America the vast majority of reported dart-throwers are of the "mixed" type as are those of Mexico.[22] However, in excavations at Roaring Springs Cave in the northern Great Basin "male" type dart-throwers have been found.[23] They differ from the usual Southwestern Basketmaker type of thrower, and should be considered for their possible homologies to the Baja California specimens. The Oregon (Roaring Springs) throwers and those discovered earlier at Lovelock Cave[24] have raised hooks carved from the same piece of wood as the shaft, but there the similarity with the Las Palmas dart-throwers ends. Both the Roaring Springs and Lovelock Cave specimens have shafts which are flat boards. The latter actually have a slight groove in the surface of the board near the hook. It appears that they are intermediate in a typological sense between a "male" type and the "mixed" type. The finger grips on the proximal ends of the better-preserved specimens from Roaring Springs Cave are provided with two grooves opposite each

19 Harrington, 1960, p. 68, plate XV,a.

20 Cushing, 1896, pp. 371-372; plate XXXII,3,4. Photographs of these specimens were also examined through the kindness of Dr John M. Goggin.

21 John M. Goggin, personal communication.

21a Swanton, 1938, p. 356.

22 See Aveleyra, et al., 1956, pp. 183-186 for a complete list of occurrences from the Southwest, Texas, and north Mexico.

23 Cressman, 1942, frontispiece.

24 Loud and Harrington, 1929, pp. 110-111, fig. 18,a.

other. Undoubtedly there were loops to enclose the grooves;[25] if so, they would have had grips like those of the Basketmaker atlatls.[26]

The Las Palmas dart-throwers with their cylindrical wooden shafts and "male" hooks, and the single bark loop grip are an entirely distinct form of "male" thrower in North America.

One must look far to the south of the Great Basin and Baja California for other occurrences of "male" types of dart-throwers. In the New World their major area of occurrence is in western South America. They are also known from northeastern Brazil[27] and among the Tainan people of the Antilles.[28] There is reason to believe that the Tainan throwers are related historically to those of Colombia.[29]

Strikingly, it is among the dart-throwers of South America that we find the closest resemblance to those of the Las Palmas Culture. The throwers of lowland Colombia, the Greater Antilles, coastal Peru, and in the Andean regions of Colombia, Ecuador, and Peru usually are round staves with a "male" hook raised on the distal end to receive the dart; the hook is either carved from the shaft or is a separate piece lashed to it.[30] However, it is specifically with dart-throwers of the Tainan peoples, and from the extreme northwestern regions of Colombia that we find precise coincidence of the round shaft, "male" hook, and a single loop grip placed to one side of the proximal end of the thrower. The single minor discordant feature in the resemblance is in the fact that the loops—at least on Haitian dart-throwers—were made of braided cotton.[31]

In view of the remarkable similarities between the throwers of these two widely separated areas of the Caribbean and southern Baja California, one must infer that they are probably historically related, since independent invention of the multiple features of similarity is unlikely. Either there was direct diffusion from northern South America—or vice versa—or both represent sporadic retention and survival—in both areas—of an ancient type of dart-thrower.

SUMMARY AND CONCLUSIONS

The "male" type of dart-thrower to which the Las Palmas examples from the

25 Cressman, 1942, frontispiece.
26 Guernsey and Kidder, 1921, plate 33.
27 Cressman and Krieger, 1940, p. 27.
28 Lovén, 1935, p. 440-445.
29 *Idem*, p. 440.
30 Cressman and Krieger, 1940, pp. 34-36; Uhle, 1909, pp. 624-627; Métraux, 1949, pp. 244-247.
31 Lovén, 1935, p. 442.

Cape region of Baja California conform seems to be the oldest and most widespread of known types. This is sustained by its antiquity in the Upper Paleolithic of western Europe, and by later ethnographic distributions in which the type is found throughout the world in extremely marginal positions. However, the time is ripe for refinement of the classification of the "male" type of dart-thrower as it appears in complexity of detail in the archaeology and ethnography of the New World.

The Baja California dart-throwers have been reported in the documents, and discovered archaeologically, only in the most southerly and remote part of the peninsula. The historical accounts of the 17th century report the weapon in use around the Bay of La Paz and on the immediate off-shore islands. This is the area held by Guaicurian-speaking Indians—Cora, Aripe, Huchití, and Guaicura at La Paz Bay—and the Pericú Isleños of the southern gulf islands[32] (Fig. 1). The archaeological specimens were recovered in southeasternmost Baja California, in the historic area of the Pericú. There is every reason to believe that the dart-thrower reported historically is typologically that of the archaeological specimens.

For all of North America the Cape region of Baja California is notable as an area in which there has been a survival of traits which are ancient elsewhere on the continent.[33] Without much doubt this dart-thrower of the "male" type represents such a survival. In Baja California, as elsewhere, it survived to be found in use side by side with the bow and arrow in the 17th century.

There is an alternative explanation which appears unlikely. Chronologically it is possible that the Caribbean type of dart-thrower, with which the Baja California type is identical, could have been introduced to the natives of the Cape region prior to 1615-1616 when it was first recorded.[34] If this was the case diffusion presumably would have been through native American crew members from the Caribbean or South America sailing with Spanish expeditions. The introduction could then have occurred during the brief contact of the Becerra Expedition (1534), the attempted colony of Cortes (1535), the Vizcaino expedition of 1596, or of his brief coastal contact in 1602. We do know that Negro divers from the Caribbean were employed, for example with Iturbe (1615-1616)[35] and with Ortega (1633).[36] On the other hand, if native Mexican crew members were responsible for introducing the dart-thrower it would certainly have been the Mexican "mixed" type, which is not found in Baja California.

32 Massey, 1949, pp. 275-287.
33 Kirchhoff, 1942; Massey, ms.
34 *Documentos Inéditos*, vol. 9, p. 35.
35 Mosk, 1934, p. 54.
36 *Col. Doc. Hist. de los Viajes*, 1944, p. 82.

Granting its chronological possibility, there are a number of reasons for the improbability of this explanation. Darts, but not the thrower, are reported before 1615. Furthermore, it is difficult to believe that this complex instrument could have been actively embraced for war purposes by peoples already familiar with the bow and arrow as were all tribes in the Cape region. Besides, the Caribbean islands were depleted of natives by the time the critical pre-1615 voyages were made, and peoples in other possible source areas had been acculturated for some time.

All of the evidence strongly suggests that the dart-thrower briefly reported by the Spanish, and recovered archaeologically, is indeed an ancient type sporadically retained in the New World, and—most notably for North America—in the cul-de-sac of the Cape region of Baja California among peoples famous for their lack of material sophistication. The type of thrower, its manufacture, and the use of bark for a finger-loop are consistent with their skills and culture.

The archaeological and documentary data have a dynamic aspect when we consider that this weapon vanished from the cultural inventory of the area between 1534 and 1643, shortly after the arrival of Europeans. Apparently the dart-thrower was on the verge of disappearance. It is fortunate that the Spanish observed the survival.

A truly remarkable aspect of the situation lies in the fact that the Las Palmas weapons are of the old "male" type rather than the "mixed" type of Mexico and the Southwest. Thus they seem to belong to a distinct technological and historical tradition. In some manner this tradition appears to involve the Archaic period and recent marginal cultures of the Southeast and certain cultures of the Caribbean and South America. Perhaps other such specific resemblances may come to light. It is probable that similar specimens will appear in excavations farther north in western North America; the Roaring Springs Cave specimen may be relative to this possibility. Yet they may belong to time levels sufficiently remote in the western United States as to be rarely preserved.[37]

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A REINTERPRETATION OF CHOCÓ SOCIETY*

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PUBLISHED ACCOUNTS of the Chocó (Panamanian and Colombian) are altogether sketchy and, in themselves, inadequate to significant sociological interpretation. Aside from some good descriptions of material culture and the incorporation of numerous myths, accounts record little more than observations of quaint custom and belief. This scanty ethnographic literature, however, has been summarized and interpreted by David Stout[1] in an article which serves as a good introduction to Chocó culture but which is quite misleading in the area of social structure.

On the basis of information obtained in the field,[2] I would like to make limited observations on the systems of kinship, marriage, and residence with specific reference to the following statement about the Chocó:

> All three groups of *Chocó* appear to be exogamous, though to what extent is not known. Aside from this exogamy, there is also obligatory exogamy in reference to patrilineal lineages which may be clans. . . . Marriage residence, ideally patrilocal, actually is alternately patrilocal and matrilocal, for women have ownership rights in some of the agricultural plots; consequently they and their husbands periodically return to the woman's parents' house to work her land. Thus each household, composed of several conjugal family units, has a constantly shifting membership.[3]

DEMOGRAPHY AND ETHNIC RELATIONSHIPS

The Chocó are a riverine people inhabiting the tropical forests of Panama and Colombia. There are possibly between 4000 and 5000 Chocó scattered along the river systems of Darién. Between 800 and 1000 of these live in the study area on the Chico River and its tributaries, between the town of Yaviza and the Serranía del Darién.

Of importance to an understanding of Chocó society is the fact that they have

* I would like to give credit to Mr Philip Young for a critical reading of the final draft of this paper, from which I benefitted substantially.

1 David Stout, "The Choco" (in *Handbook of South American Indians,* J. H. Steward, ed., Bulletin 143, Bureaul of American Ethnology, vol. 4, pp. 269-276, 1948).

2 The field research which has made this paper possible could not have been accomplished without the generous assistance of the National Science Foundation. During the research period, the summer of 1960, two weeks were spent on the Chico River in a place known as El Naranjal. Here I obtained my most detailed and quantified information.

3 Stout, *op. cit.,* p. 273.

been migrating north from Colombia, apparently for centuries. They explain this migration as the result of a desire for new and adequate lands, stimulated by the pressure of an impinging Negro (*colombiano*) population. The *colombianos* are also moving north and are displacing Chocó, not only in Colombia but also from their centers of earlier settlement in Panama—the lower reaches of the main rivers and larger tributaries.

Interspersed among the riverine houses of the Chocó are those of *colombianos* and (a few) *panameños* (predominantly negroid nationals of Panama). A rounded study of the Chocó would of course include a detailed appraisal of relationships with these Negro peoples, but, for present purposes, the most important feature of the relationship may be stated succinctly. It is simply that Chocó do not marry with other peoples, Cuna included. They are an ideally ethno-endogamous society, and there seems to be a nearly one hundred percent approximation to this ideal. Even in the Chocó "settlement" in Panama City, there is not much evidence of miscegenation. *Panameños* and *colombianos* maintain that they are able to live in close harmony with Chocó because they neither molest nor attempt to marry Chocó women. While there is significant interaction between Chocó and Negro (economic, magico-religious, recreative), Chocó endogamy and its corollary, the notion of "racial distance," serve to maintain ethnic integrity among this small and shifting population.

GREATER CHOCÓ SOCIETY

When Stout described "all three groups" as exogamous and contrasted *this exogamy* to obligatory exogamy in patrilineal descent groups, he posed a problem insoluble of interpretation. While it is inconceivable that he meant each "group" was exogamous (with reference to the two other groups?), we are, nevertheless, faced with defining two kinds of exogamic units. It is likely that Stout merely meant that a rule of exogamy is in evidence among all Chocó and that one might at least be certain that a partial definition of this rule could be made with reference to patrilineal lineages. We are confronted here, however, with more than unfortunate phrasing. Rather, this kind of statement indicates a misapplication of theory. In any case, there are no patrilineages in Chocó society.

A further difficulty of Stout's summary is that the concepts of culture and society are not always clearly distinguished. Among "all three groups" are included the Colombian Catío of the Urubá region, fronting on the Caribbean. We have then the Northern (Panamanian) and Southern (Colombian) Chocó along with the Colombian Catío in some sort of triad. Presumably, this identification is made

on the basis of Severino's description of the Catío.[4] It is my impression that the Catío do not comprise a segment of greater Chocó society. While there are obvious cultural similarities between Chocó and Catío, there are no indications of sustained social intercourse between them—and the cultural similarities, aside from language, seem no greater than those between Chocó and Cuna. But there is really no adequate information about Catío society and whatever relationships might exist with other societies, and, therefore, no justification on sociological grounds for their inclusion among the Chocó. Whatever their relationship with the Chocó, it remains to be discerned.

I would, however, classify the Northern and Southern Chocó as comprising a single society, although this has not been done heretofore. Stout lumps them culturally, but this is a classification of a different order. Wassén[5] distinguishes between True and Nonomá Chocó (in Colombia), primarily on the basis of dialect differences. With respect to kinship terminology, dialect differences certainly exist, but they in no way alter the pattern of nomenclature. At least three such differences were noted along the Chico River and seem to be a fairly widespread phenomenon, understandable as a result of continual population movement and social interaction.

It seems to me that this is a datum of great value in suggesting that the kinship system is essentially the same for both Panamanian and Colombian Chocó. I make this point simply because the customary ethnological division of the Chocó into northern and southern populations seems to deny it. Wassén's description of household composition and domestic relationships among (atypical) Colombian Chocó families[6] holds, as far as it goes, for Panamanian Chocó. My field data are also in agreement with Stout's observation that persons make extended visits between the two areas.[7] There is evidence, therefore, that the social systems in both areas are alike and that there is sustained social intercourse between both areas.

If it were not for the international boundary and the watersheds with which it tends to coincide, it is unlikely that ethnographers would have made more than a passing geographical distinction between the Northern and Southern Chocó. However, they have seen fit to make cultural distinctions between them and have not seriously considered the area of social relationships. The cultural differences are slight, and those which exist are in evidence among Colombian Chocó in their

4 (Fr.) Severino de Santa Teresa, *Creéncias, ritos, usos y costumbres de los indios Catios de la prefectura apostólica de Urubá* (Bogotá, 1924).

5 Henry Wassén, *Notes on Southern Groups of Chocó Indians in Colombia* (Etnologiska Studier, no. 1, pp. 35-182, Göteborg, 1935).

6 *Idem*, pp. 43-44.

7 *Op. cit.*, p. 271.

native habitat and do not, therefore, serve to distinguish Northern from Southern Chocó. Since there is no evidence from the ethnographic literature and none from my own field notes of this kind of separateness, I feel that the generalizations made in this paper hold for both Panamanian and Colombian Chocó. I suggest that they comprise a single society throughout which kinship terms (unavailable to Stout when he summarized the material) and institutionalized behavior are generally extended.

STRUCTURAL CORRELATES OF THE TERMINOLOGICAL SYSTEM

Kinship terminology in use both in Panama and Colombia (dialect differences not indicated) appears on the accompanying ego-chart (Table 1), and is identifiable as Hawaiian. According to Murdock,[8] the Hawaiian type system correlates

TABLE 1

Chocó kinship terminology with reference to Ego*

I.	*Imberana:* exogamic group	
	GFs, GFBs, GMBs	*zawané*
	GMs, GMZs, GFZs	*pakoné*
	F	*dáta*
	M	*pápa*
	FBs, MBs, FZh, MZh	*droa*
	B, Z, c-Cs, p-Cs	*jabá*
	Bch, Zch	*teábea*
	S	*hurra*
	D	*kau*
	Sw	*hurra kima* or *haingú*
	Dh	*kau kima* or *bigú*
	gch	*áinzake*
II.	Recognized cognates: marriage restricted by age only	
	c-Cs sp, p-Cs sp	*jabá kima*
	c-Cs ch, p-Cs ch	*teábea*
	c-Cs ch sp, p-Cs ch sp, Bch sp, Zch sp	*teábea kima*
	c-Cs gch, p-Cs gch, Bgch, Zgch	*áinzake*

* Term for h (husband) and w (wife) is *kima*. There is little or no distinction between vocative and referential terminology, but terms are not always used in direct address. Explanation of symbols: GFs means grandfathers; Z stands for sister; ch stands for children; c-Cs stands for cross-cousins; sp stands for spouse; gch stands for grandchildren. The same terms are used whether Ego is male or female, regardless of age.

8 George Peter Murdock, *Social Structure* (New York: Macmillan Co., 1949).

significantly with the presence of kindreds or demes, groups in which sibling terms tend to be extended to include cross- and parallel cousins. In addition, such societies are characterized by the limited appearance of polygyny, the bilocal extended family, and the bilateral extension of incest taboos.

Allowing for important qualification,[9] one may discern these diagnostic criteria among the Chocó. For the purposes of this paper, however, the feature of most importance is that of the negative correlation of Hawaiian terminology and exogamous unilineal kingroups[10]—a statement of probability with respect to which the Chocó serve as a case in point.

There is an infinite extension of kinship terms among the Chocó, whether taken as a local sector, a regional (river system) population, or an ethnic totality. When kinship terms are used in addressing non-relatives they conform to an etiquette of generational difference or similarity with regard to the status of two persons. Uncle, nephew, and sibling terms are widely used in this way. They are *always* used among co-residents of any sector, *generally* among persons living along any river and, whenever salutations are made, *frequently* among all Chocó. In a sense, all Chocó are kin. Kinship terms are not extended to non-Chocó. Creation myths identify all Chocó as "people," sometimes in specific distinction to both Negro and Cuna who, in an ultimate social sense, are not "people."

The "equivalence" of siblings implied by the terminology exists only in the very general sense of status equivalence with respect to generation membership. It does serve to symbolize limited solidary relationships among generation mates within any sector of intermarrying households, as well as the distinction between contiguous generations (both on the level of the sector and the household) with regard to daily social intercourse. This is of considerable importance for the incorporation of new members into any residential nucleus. But it in no way implies life-long or long-term coöperation in a specific activity among members of any terminological category, with or without regard to residence, full siblings included.

The actors shown on the ego-chart may constitute a segment (kindred) of the local sector which, however, falls short of achieving *community* status to the extent that the ideal of local group (deme) endogamy is not fully realized, and the residents of the sector engage in no corporate activities. As shown on the chart, the extension of sibling terms to first cousins correlates with the definition of the incest group *(imberana)*. But the terms themselves really do not indicate this

9 These are qualifications of the sort which will be discussed in a forthcoming paper dealing with marriage, residence, and the domestic cycle among the Chocó of Darién, and which will consider certain institutions in the light of "economic change" and structural continuity.

10 Murdock, *Social Structure*, p. 228.

definition, since they are extended far beyond this genealogical unit—extended as status terms of another order. One is forbidden to marry a person who stands in a relationship closer than that of second cousin. It is said that this rule is never broken, and my data do not indicate its infraction. Beyond the lateral category of first cousin, and the lineal categories of Bch, Zch, S, D, gch and, if children have been born to them, Sw and Dh, therefore, the concept of "family," "close relatives," "kindred" (*imberana*) does not apply, and genealogies are usually not known with any degree of accuracy. I take this extension of incest to define the kindred in Chocó (or any) society and, since I am especially concerned with Stout's observations, to indicate an absence of exogamous unilineal kin groups.

The fact that genealogical connection is imperfectly known beyond cousins of the second degree makes research in this area of relationships most difficult and time consuming. For example, in any local sector there are almost no grandparents (having nubile or married grandchildren) comprising a significant social category. With respect to marriageable persons, this is a two-generation society, from all indications. Because of short life span on one hand and physical mobility on the other, connections between sectors along the same river are difficult to ascertain with regard to consanguineal and affinal bonds, and possible connections of this sort which stretch across entire river systems are impossible to verify by means of standard ethnographic techniques. Add to this difficulty the fact that the Chocó (in distinction to other Spanish-speaking indigenous peoples) carry only the paternal surname, and the verification of inter-sector marital relationships appears most difficult. As difficult as this is for the genealogical reckoning of the anthropologist, it must be recognized as of great social significance for the Chocó. It correlates with notions of (1) *imberana*, in which marriage is proscribed between persons classed as within the exogamic group (cf. Table 1); with (2) "distantly related" segments of the recognized cognatic group (cf. Table 1), and, negatively, with the existence of patrilineages and clans.

RESIDENCE

The final point on which I wish to comment is Stout's remark about post-marital residence patterns. This will not lead to an understanding of relationships surrounding marriage—relationships which I will discuss elsewhere in connection with the developmental cycle of the domestic group. Here I merely indicate that a consideration of the developmental cycle is crucial to a full understanding of Chocó society. For present purposes it suffices that the domestic group *(de)* invariably undergoes compositional change. At one time or another it may consist of contingent ele-

mentary families in temporary uxorilocal or, rarely, temporary patrilocal residence. The cycle begins and ends with an elementary household. Any household, however, regardless of its composition is always a patripotestal group, in the specific sense that the owner of the house is its leader *(paterfamilias)* and the person who allocates the household resources among the membership. With this in mind I want to consider the possible conceptual route which led Stout to misinterpret the structure of Chocó society, especially with reference to patrilineages and clans.

The localized nucleus (the sector) of households connected by blood and/or marriage (characteristic of Chocó society) seems to have been observed by Stout and was interpreted as being comprised of localized segments of intermarrying lineages (or clans). If the authority principle of *patria potestas*, clearly observable in the domestic group, were to be confused with the supposed principle of patrilineal descent, then—but only in the absence of genealogies—residential nucleii might appear to have some semblance of localized sets of lineages or clan segments. Investigation of this possibility bore fruit, but the hypothesis itself was not confirmed. Neither patrilineages nor patriclans exist in Chocó society.

For one thing, residence is neither "ideally patrilocal" nor actually "alternately patrilocal and matrilocal." Residence patterns do not contribute to the definition of unilineal groups. Patripotestality is a defining principle only within the household *(de)* and father-right does not extend to the households of offspring in permanent post-marital residence. With the marriage of offspring, and especially after the birth of their first children, households fragment into virilocal units, the locations of which are largely determined by the availability of plantain land and the "failure" of the verbalized bilateral (inheritance) ideal. This is a statement about contemporary Chocó society, but it seems applicable to by-gone days as well. In earlier times men needed ample territory (as they do today in areas which are not overwhelmingly cash-crop oriented) in which to hunt; now they need ample land on which to raise plantains. Certain values and goals have changed but adaptations to these economic changes have not so much altered as perpetuated the basic structure of Chocó society. Integral to this structure is the rule of virilocal residence and its observance. The availability of plantain groves helps determine *where* the virilocal household will be established, with reference to one's natal group, but there is a nearly one hundred percent observance of the virilocal rule. Because plantain land (riverine land with sandy soil) tends to become occupied within two generations in any local sector, some of a man's offspring (and their grown children) have to move outside the sector to establish households after marriage.

Chocó residence patterns are best viewed with respect to the degree of observ-

ance to a *rule* of residence. There is both observance and patterned exception, although the observance is nearly complete. Rather than the rule being patrilocal, it is virilocal following a period of uxorilocality. When patrilocality occurs, either temporarily or permanently,[11] it is a departure from the rule. If patrilocality were the rule, married sons would be found in their natal household with their in-married wives and children. Unless there were some feature of household organization which brought about fission or segmentation at the second generation, then one might also expect to find households occupied by cousins and their families. None of this occurs among the Chocó. Even as a temporary arrangement, patrilocality appears to be quite rare, not a single case being found in the study area.

The "matrilocality" suggested by Stout is a totally useless and misleading concept for the analysis of Chocó residence and relationships contingent on marriage. It implies a structure which is wholly foreign to Chocó society. It emphasizes a solidary relationship achieved through alignment with the distaff side of a domestic group. The temporary return of a woman and her husband to her *father's* house in order to harvest or care for fields of her own could by no stretch of the definition be considered an expression of a matrilocal rule. And since, in this "bilateral" society, women rarely inherit life-long rights to plantain land, and since the cultivation and, especially, the harvesting of plantains (the only holding of consequence) necessitates the residential proximity of those who do the work, the notion of alternate residence (of any sort) and the "constantly shifting membership" of a household is inconsistent with the structure of Chocó society.

As an outgrowth of the simple marriage ceremony, which consists of an acceptable young man sleeping a few nights with his bride in her father's house, there is a period of temporary uxorilocality. This period is usually terminated about the time of the birth of the couple's first child, and ideally coincides with the groom's readiness to harvest plantains from his own grove. After this, the young couple establish a permanent virilocal household. This completes the uxorilocal-virilocal continuum in Chocó society and constitutes the *rule* of post-marital residence. Because of temporary uxorilocality (a phase in the cyclical development of the domestic group) and because of a misinterpretation of cultural norms, it is possible that Stout was led to view household composition as "shifting" in membership. A good deal of visiting occurs, involving friends as well as relatives, and shelter is offered visitors as a matter of form, but it is unlikely that Stout was referring to this pattern in a consideration of rules of residence.

Beyond the obvious fact that there are no patrilineages or patriclans—social

11 It is never permanent with respect to father's house, since the dwelling is abandoned when the paterfamilias dies, at which time a new house is built elsewhere on the holdings.

features determinable only in the field—my disagreement with Stout's interpretation of Chocó social structure is no mere quibbling with words. It is an effort to be more specific with regard to concepts which express Chocó social relationships. I object on principle to overworking the concept of "patrilocality" and to misapplying that of "matrilocality." This lack of precision has led to unfortunate confusion in a number of papers and monographs and, in the case of the Chocó, stimulated the research on which this paper is based. The possible confusion between principles of authority (patri-potestality) and descent (patrilineality) is, given the state of Chocó ethnography, rectifiable only in the field. But, the jumbling of principles such as patrilineality, patripotestality, patrilocality, and matrilocality seems a characteristic of "reconstructions" based on poor source material to which inadequate sociological theory has been applied.

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